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PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

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Session 1866–7.

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NOTE.

The object which the Society have in view in publishing their Proceedings, is to give an immediate and succinct account of the scientific and other business transacted at their meetings, to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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Report of the Council.—April 30, 1867, p. 165.

ERRATA.

Page 134, line 22, for θ and θ_1 in the equation, read *tan* θ and *tan* θ_1.

Page 136, line 9, for *generally* read *supposed to be.*

PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 2nd, 1866.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Among the donations announced was one from Mr. Cottam, of a framed Memorial, presented by the Society to Dr. Dalton in 1844, on the occasion of his communicating to them the 50th annual result of his meteorological observations. In a letter accompanying the memorial, dated the 5th September, 1866, Mr. Cottam says: "At the Doctor's death it remained in the hands of our old friend, Peter Clare, at whose sale it was purchased by my late father, and has since hung up in my office as a fine specimen of caligraphy. I think the Society's rooms will be the most suitable place for it, and that the centenary anniversary of the Doctor's birth is a suitable occasion for its presentation."

On the motion of Mr. BINNEY, seconded by Mr. GASKELL, the thanks of the Society were unanimously voted to Mr. Cottam for his interesting donation.

A paper was read "On the Portraits of Sir Isaac Newton; and particularly on one of him by Kneller, painted about the time of the publication of the *Principia*, and representing him as he was in the prime of life," by SAMUEL CROMPTON, M.D.

The writer pointed out that all Newton's great discoveries were made prior to 1688, and directed attention to the following dates: That he was born in 1642, died in 1727, and

that he discovered Fluxions in 1665-6, the Unequal Refrangibility of Light in 1666, the Reflecting Telescope in 1668, and published the *Principia* in 1687; and that with that publication, which Whewell has designated "The Great Charter of Modern Science," his career, as an original discoverer, appears to have terminated. He quoted passages from Mr. Edleston's Correspondence of Newton and Cotes, and from Sir David Brewster's larger Life of Newton, to show that his appointment to the Mint, in a great measure, stopped further researches. "Henceforward," says Mr. Edleston, "his official duties rendered it impossible for him to work continuously at his former pursuits; and his studies in mathematics and natural philosophy were by snatches, and in the intervals of business." "His duties at the Mint," says Sir David, "allowed him but little time for the performance of so laborious a task as the publication of a second edition of the *Principia*, and when his consent was at last obtained to put the work to the press, they greatly interrupted its progress?" Again, in one of his letters to Flamstead, he says that he did not like to be known to be attending to anything but the King's business, meaning the duties of the Mint. Therefore, after the publication of the *Principia*, he may be practically regarded as a retired philosopher.

The writer then attempted to show that the current portraits of Newton do not give us any correct representation of Newton, the philosopher. He exhibited various engravings of Newton. He pointed out that Vertue's print, after Vanderbank, is inscribed "Isaacus Newtonus Eq. Aur. æt. 83. J. Vanderbank pinxit 1725, Geo. Vertue sculpsit 1726;" and, therefore, that the date of that celebrated picture, in possession of the Royal Society, represents him in extreme old age. This picture, therefore, he contended, was not a portrait of the great Newton, but of Newton the Master of the Mint, who assayed Wood's halfpence; and that it could not, for one moment, be accepted as a representation of

Newton in the plenitude of his intellectual power, of whom Halley sang in 1687:—

> Newtonum clausi reserantem scrinia veri,
> Newtonum Musis charum, cui pectore puro
> Phœbus adest, totoque incessit numine mentem,
> Nec fas est propius mortali attingere divos.

Nor was it the portrait of him as he was when he published the second edition of his *Principia*, but a representation of him as a grand and venerable ruin covered with the lichens of time.

But this picture of Vanderbank's, the writer greatly preferred to the Houbraken and other prints published about 1710 or 1712; and he contended that the portrait published, as the best extant one, by Sir David Brewster, as a frontispiece to his larger Life of Newton, by no means gives a desirable representation of Newton the philosopher. It was rather an affected representation of Newton the dandy, and of Newton the prosperous man of the world, with a carriage and horses, and with three male and three female servants. The writer looked upon these prints with pity; and could not, for one moment, allow that any one of them represented that Isaac Newton, the yeoman's son, while at work in the wells of truth, and wresting from nature secrets hidden from the foundation of the world. In order to form some idea of the immortal Newton, as distinguished from Queen Anne's Newton, he asked us to consider attentively the letter written by Newton's former assistant, Humphrey Newton, to Mr. Conduit, in 1727, descriptive of Isaac's manner of life and appearance between 1684 and the beginning of 1689.

"In the last year of King Charles II., Sir Isaac was pleased, through the mediation of Mr. Walker, then schoolmaster at Grantham, to send for me up to Cambridge, of whom I had the opportunity, as well as honour, to wait of for about five years. In such time he wrote his *Principia Mathematica*, which stupendous work, by his order, I copied

out before it went to the press. . . . . His carriage, then, was very meek, sedate, and humble, never seemingly angry, of profound thought, his countenance mild, pleasant and comely. I cannot say I ever saw him laugh but once which was at that passage which Dr. Stukeley mentioned in his letter to your honour*, which put me in mind of the Ephesian philosopher who laughed only once in his life time, to see an ass eating thistles when plenty of grass was by. He always kept close to his studies, very rarely went a visiting, and had as few visitors. . . . . I never knew him to take any recreation or pastime either in riding out to take the air, walking, bowling, or any other exercise whatever, thinking all hours lost that was not spent in his studies, to which he kept so close that he seldom left his chamber except at term time, when he read in the schools as being Lucasian professor, where so few went to hear him, and fewer that understood him, that oft times he did in a manner, for want of hearers, read to the walls. . . . . So intent, so serious upon his studies, that he ate very sparingly, nay, ofttimes he has forgot to eat at all, so that, going into his chamber, I have found his mess untouched of which, when I have reminded him, he would reply,—"Have I?" and then making to the table, would eat a bit or two standing for I cannot say I ever saw him sit at table by himself . . . . He very rarely went to bed before two or three of the clock, sometimes not till five or six, lying about four or five hours especially at spring and fall of the leaf, at which times he used to employ about six weeks in his laboratory, the fire scarcely going out either night or day, he sitting up one night and I another till he had finished his chemical experiments, in the performance of which he was most accurate, strict, exact. What his aim might be, I was not

* When Sir Isaac once laughed 'twas upon occasion of asking a friend to whom he had lent Euclid to read, what progress he had made in that author, and how he liked him? He answered by desiring to know what use and benefit in life that study would be to him, upon which Sir Isaac was very merry.

able to penetrate into, but his pains, his diligence at these set times made me think he aimed at something beyond the reach of human art and industry. I cannot say I ever saw him drink either wine, ale, or beer, excepting at meals, and then but very sparingly. He very rarely went to dine in the hall, except on some public days, and then if he was not minded, would go very carelessly, with shoes down at heels, stockings untied, surplice on, and his head scarcely combed. . . . . When he has sometimes taken a turn or two (in his garden) he has made a sudden stand, turned himself about, run up y^e^ stairs like another Archimedes, fall to write on his desk standing without giving himself the leisure to draw a chair to sit down upon. . . . . His brick furnaces, pro re nata, he made and altered himself without troubling a bricklayer. . . . . He very seldom sat by the fire. . . . . He never slept in the day time that I perceived: I believe he grudged y^e^ short time he spent in eating and sleeping. . . . . His bed maker in a morning has sometimes found both dinner and supper scarcely tasted, which the old woman has very pleasantly and mumpingly gone away with. . . . . His thoughts were his books; though he had a large study seldom consulted with them. When he was about 30 years years of age his grey hairs was very comely, and his smiling countenance made him so much the more graceful."

Surely, Dr. Crompton contended, none of the engraved portraits of Newton represents the great Newton of those years. The conceited, double-chinned Kneller's of 1710-20, give no idea of him. Dr. C. then exhibited an admirable engraving by Mr. Oldham Barlow,* of the recently recovered portrait of Newton, painted by Kneller in 1689, and asked whether it did not fulfil every required condition? Eyes capable of penetrating into the heart of things—a brow that could measure the universe—a face "full of Godlike reason" and in its intensity and force indicating a very beagle of

* Of Victoria Road, Kensington, London.

truth—yet what simplicity! and how contemplative! The identical white locks spoken of by Humphrey Newton, to be in a few years shorn to make way for the wig of the Houbraken print!

Dr. C. then gave the history of the portrait from whence this engraving has been taken, and said he identified it at the Art Treasures' Exhibition, as Newton as he was when he wrote the *Principia*; and pointed out at that time that it was the only known portrait of Newton worthy of notice—that he obtained, in conjunction with Dr. Joule, Mr. Fairbairn, Dr. Schunck, Mr. Binney, and Dr. Angus Smith, leave from its noble owner to have it photographed; and he exhibited a print then taken—that Mr. Barlow happening to be at his house, saw an impression of it, and expressed a wish to engrave it—that Dr. Crompton obtained Lord Portsmouth's permission for the use of the original picture. Dr. C. then read letters from Lord Portsmouth, proving that the portrait came into his lordship's possession by the marriage of his ancester Lord Lymington, to the grand-niece of Newton, Miss Conduit, and that it had been in a neglected condition and recently cleaned and lined. Dr. C. added that Sir David Brewster told him that he went to Hurstbourne Park to examine Newton's MSS. and portraits, but that he did not see it, yet it was there. The picture, Dr. C. added, is in Kneller's best manner, is painted with great care, is signed in the left corner "Isaacus Newtonus," and in the right, "Godfrey Kneller fec. 1689." Dr. C. exhibited an engraving prefixed to Dr. Edleston's Correspondence of Newton and Cotes, which he said was from an India ink drawing in Magdalen College, Cambridge, undoubtedly closely allied to the Kneller picture, but greatly inferior; being, in Dr. Crompton's opinion, a bad copy by an inferior hand, but, no doubt, made for Mr. Pepys, in whose collection it was.

Dr. C. showed that Newton was in London during the

greater part of 1689, as Member of the Convention Parliament, when it is certain that this portrait was painted; and he added, that he had not been able to discover any other portrait of him painted earlier than, or about, this interesting period of his life, nor any other, till several years later (probably twenty), when success had greatly changed his countenance.

PHOTOGRAPHICAL SECTION.

October 4th, 1866.

Dr. J. P. Joule, F.R.S., &c., Vice-President of the Section, in the Chair.

A paper was read "On a Process for Printing Photographs in Various Colours," by J. A. Gatty, Esq.

My process is based upon the property possessed by ferricyanide of potassium, of forming clear solutions with certain metallic salts, producing insoluble compounds when the mixture is brought into contact with a deoxidising agent, the rays of the sun acting as such, a perfect precipitation takes place upon paper or other material prepared with the above-named solution. In producing the specimens sent herewith, I applied to the paper a concentrated solution, formed of equal parts of ferricyanide of potassium and nitrate of lead, having found the latter to answer very well, not only as a means of forming a precipitate, but also for assisting in the production of numerous colours. After drying the paper, it was exposed to the sun for about half an hour, and then washed in water in order to dissolve all the unaffected ferricyanide of potassium and nitrate of lead. I have noticed that the sun acts much quicker when there is a little moisture present. I have, therefore, placed a damp cloth between two or three thicknesses of paper behind the prepared paper.

After washing, the Photographic image remains behind as a pale greenish precipitate, easily transformed into various colours, as the following experiments will show:—

No. 1. (Blue.) Has been steeped in a weak solution of nitrate of iron for about ten minutes, and then washed in water.

No. 2. (Green.) Same as No. 1, but steeped in a weak solution of bi-chromate of potash after the nitrate of iron.

No. 3. (Reddish Brown.) Has been steeped in a solution of nitrate of copper, and then washed.

No. 4. (Brown.) Has been developed by steeping it in a mixture of weak solution of nitrate of iron and nitrate of copper.

No. 5. (Dark Brown.) Has also been treated with nitrate of iron and nitrate of copper, but containing a larger proportion of the former.

These few experiments will show that a very large number of shades may be obtained by using different salts and mixtures thereof in developing the Photograph. A further series of colours may be obtained by destroying the blue with caustic soda, which, after washing, will leave behind oxides of iron and lead, which may be dyed with vegetable colouring matters.

All the above experiments have been made about four years ago, which goes to prove that the colours are permanent. I hope shortly to be able to resume my experiments, and work the process out more perfectly.

Mr. Dancer remarked that he had some prints (which were shown to the meeting) produced by Mr. Mercer in 1856, which appeared to be done by a process similar to the one just described, prussiate of potash being one of the substances used, but he was not acquainted with the details of the process.

It was considered a useful process for certain purposes, and deserved more attention than it had hitherto received.

Ordinary Meeting, October 16th, 1866.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On the Isomorphism of Thallium-perchlorate with the Potassium and Ammonium-perchlorates," by Prof. H. E. ROSCOE, F.R.S., &c.

The recently ascertained isomorphism of the Thallium- and Ammonium-sulphates* renders it highly probable that the perchlorates of these metals also exhibit isomorphous relations.

Thallium-perchlorate equals the Alkaline-perchlorates in stability. It can be readily prepared either by dissolving metallic Thallium in aqueous Perchloric Acid, or by the double decomposition of Thallium-sulphate and Barium-perchlorate. From solution the anhydrous Salt is easily deposited in colourless rhombic crystals, which are transparent, bright, well-defined, and non-deliquescent. The specific gravity of the crystals is 4·844 at 15°·5 C.; they dissolve in 10 times their weight of water at 15°, and in about $\frac{3}{5}$ths of their weight of water at 100°; the salt is also slightly soluble in alcohol. Thallium-perchlorate does not lose weight when heated to 200° C., and the temperature may even be raised to

* Lang, Phil. Mag., xxv. 248.

within a few degrees of the boiling point of mercury without producing the slightest decomposition of the salt. On the further application of heat, a black mass is formed, and the salt finally volatilizes as Thallium-chloride.

The crystalline form of Thallium-perchlorate is that of a right rhombic prism (pp_1), in which the faces of the rhombic octohedron (rr_1) and the basal faces of the prism (c) generally appear; the crystals being lengthened, as is the case with the Alkaline-perchlorates sometimes in the direction of the prismatic, and sometimes in the direction of the octohedral faces. A careful measurement of the crystals gave the mean value of 102° 50′ for the angle pp_1; and that of 102° 6′ for the angle rr_1 on c. Hence the relation of the axes is 0·7978 : 1 : 0·6449.

The angles observed by Mitscherlich,* in the case of Potassium- and Ammonium-perchlorates agree exactly with the above; for the first of these salts pp_1=103° 58′, rr_1 on c= 101° 19′; and for the second pp_1=103° 11′, and rr_1 on c= 102° 4′, giving the relation of the axes to be (1) 0·7817 : 1 : 0·6408 and (2) 0·7926 : 1 : 0·6410. The following analysis, made in my laboratory by Mr. T. E. Thorpe, shows that the formula of the salt is $Tl\,Cl\,O_4$.

(1) Determination of Thallium. The crystals were well dried in vacuo over Sulphuric Acid, and precipitated with Platinum-tetra-chloride, the passage of the finely divided precipitate through the filter being avoided by evaporating to

* Pogg. Ann. xxv. (1832), p. 301.

dryness on the water bath, and taking up with absolute alcohol.

(a) Salt prepared by direct solution of the metal in Perchloric Acid; 0·1831 salt yielded 0·2476 Chloroplatinate.

(b) Salt prepared by double decomposition; 0·4502 salt yielded 0·6060 Chloroplatinate.

(2) Determination of Perchloric Acid. A solution of Potassium-acetate was added to the Thallium-perchlorate, and the whole evaporated to dryness on a water-bath, the acetates of Thallium and Potassium washed out with absolute alcohol, and the insoluble Potassium-perchlorate collected on a weighed filter; 0·4570 Thallium-perchlorate yielded 0·2100 Potassium-perchlorate. Hence:

	Calculated.	Found.		
		I.	II.	III.
Tl.....204·0	67·21	67·38 (a)	67·18(b)	
Cl..... 35·5	21.09			} 32·96
O_4..... 64·0	11·70			
303·5	100·00			

"Catalogue of Binary Stars, with introductory Remarks," by A. Brothers, F.R.A.S.

The Author said that the purpose for which he had prepared a list of the stars known to be Binary at the time of the publication of Smyth's "*Cycle*," was to bring down the information as to the positions and distances of those stars to the present time, and for this object he had obtained the necessary information from some of the most eminent observers. He had found much inconvenience from having to examine a number of catalogues for the information he required for his

own observations, and in the list submitted he had brought into one view all that the amateur would require in prosecuting his researches on the Binary Stars.

Mr. Brothers said that he thought it unadvisable to include in the present list any other objects than those which had been observed by Smyth, and that he intended to prepare another list, which would include most of the stars which had shown a sufficiently large change of angle of position since 1842, to entitle them to a place with the well known Binary systems.

The Author's Catalogue will shortly be published by the Society.

PHYSICAL AND MATHEMATICAL SECTION.

October 11th, 1866.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"Observation of the Eclipse of the Sun, October 8th, 1866, at Mr. Worthington's Observatory, Crumpsall," by JOSEPH BAXENDELL, F.R.A.S.

The first contact was observed at 4h. 20m. 6·0s. Greenwich mean time. Owing to the low altitude of the Sun, its limb was very tremulous, but the error arising from this cause was estimated not to exceed one second. The telescope used was the equatorially-mounted achromatic, of 5 inches aperture and 70 inches focal length, with a positive eyepiece magnifying 68 times. The position of the observatory is, latitude 53° 30′ 50·0″ N., longitude 8′ 56·16″ W.

"Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of Sir John Herschel," by GEORGE KNOTT, F.R.A.S. Communicated by Mr. Baxendell.

Some few months ago I had the honour of submitting to the Physical and Mathematical Section of the Manchester Literary and Philosophical Society the results of a comparison of the Star Magnitudes of the *Bedford Catalogue* with those of the *Mensuræ Micrometricæ* and the *Bonner Sternverzeichniss*. In the following paper, which may be regarded as supplemental to that just referred to, I propose giving the results of a similar comparison of the magnitude scale of the *Bedford Catalogue* with that of Sir John Herschel.

The Stars for comparison are taken from Sir John Her-

schel's six Catalogues of Stars observed with the 20 foot reflector, and his two Catalogues of Double Stars measured with the 7 foot equatorial, which are printed in the earlier volumes of the Memoirs of the Royal Astronomical Society. The method of procedure was the same as that adopted in the former investigation. The magnitude assigned by Sir John Herschel to each star common to the two observers was tabulated under the magnitude assigned to it by Admiral Smyth, and the mean taken. The results thus obtained were equalized by Sir John Herschel's formula (Mem. R.A.S., vol. iii, p. 179) and the corrected and uncorrected results, together with the number of comparisons on which each depends, are given in the following Table.

TABLE.

Mag. Sm.	Mag. H.	No. of Obs.	Mag. H. Corrected	No. of Obs.
1	1·05	6	1·05	6
1·5			1·62	8
2	2·20	5	2·25	2
2·5	2·7	3	2·59	15
3	3·0	10	3·04	23
3·5	3·6	10	3·67	45
4	4·3	25	4·28	53
4·5	4·9	18	4·87	75
5	5·4	32	5·28	88
5·5	5·6	38	5·85	135
6	6·5	65	6·40	132
6·5	7·0	29	6·98	135
7	7·4	41	7·36	112
7·5	7·7	42	7·76	136
8	8·1	53	8·18	125
8·5	8·8	30	8·76	120
9	9·5	37	9·47	85
9·5	10·2	18	10·05	79
10	10·5	24	10·44	60
11	11·1	18	11·25	56
12	11·9	14	11·72	58
13	12·3	26	12·48	55
14	13·2	15	13·27	53
15	14·4	12	14·46	40
16	15·9	13	15·90	13

If we combine these results with those contained in my former paper, we obtain an indirect comparison of Sir John Herschel's scale with that of Professor Struve. The results thus obtained present but a moderate accordance with those of Sir John Herschel's direct comparison, as given in Vol. iii. of the Memoirs of the Royal Astronomical Society. This is perhaps owing, apart from the indirectness of the method, to the fact that I have not confined myself to the list of stars employed by Sir J. Herschel, and that my comparison was made with the magnitudes of the *Mensuræ Micrometricæ*, and not with those of the *Dorpat Catalogue*.

"Note on the Combined Magnitude of two Stars in close proximity," by GEORGE KNOTT, F.R.A.S. Communicated by Mr. BAXENDELL.

It is sometimes convenient to know the magnitude resulting from the combined effect of two stars in such close proximity as to appear *as one star*, either to the naked eye, or in a telescope of insufficient power to separate them.

If m and $m+\Delta m$ be the magnitudes of two stars so circumstanced, and ρ the *decreasing* 'light-ratio,' then if μ be the combined magnitude required, it may easily be shown that*:—

$$\mu = m + \frac{log.\ (1 + \rho^{\Delta m})}{log.\ \rho}$$

It thus appears that if we tabulate the values of the function $\frac{log.\ (1 + \rho^{\Delta m})}{log.\ \rho}$, (the sign of which it will be observed is essentially negative,) for certain definite values of Δm, we can obtain, by inspection, the value of the correction to be applied to the numerical magnitude of the greater of two stars closely juxtaposed, in order to obtain the magnitude resulting from their combination.

* I have adopted the symbols of the formula as kindly communicated to me by Lieut. Tupman, R.M.A., F.R.A.S., as being more symmetrical than those of my own independent investigation.

The following Table has been calculated on the assumption that $\rho = 0{\cdot}398 = \frac{1}{2{\cdot}512}$, the reciprocal of Mr. Pogson's value for the 'light-ratio.'

TABLE.

$\Delta m.$	$\frac{log.(1+\rho^{\Delta m.})}{log.\ \rho.}$
0·0	— 0·75
0·5	0·53
1·0	0·36
1·5	0·24
2·0	0·16
2·5	0·10
3·0	0·07
3·5	0·04
4·0	— 0·03

"Catalogue of Binary Stars, with Introductory Remarks," by A. BROTHERS, F.R.A.S.

[This paper was afterwards read at the Ordinary Meeting of the Society, held October 16th, 1866. See page 11.]

Erratum.

Proceedings Lit. & Phil. Society, Vol. 5, p. 127, lines 2 and 3 from bottom, for July 17·0 and December 2·6, read July 9·5 and November 24·1.

Ordinary Meeting, October 30th, 1866.

R. Angus Smith, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Professor R. B. Clifton, M.A., F.R.A.S., late of Owens College, but now of Oxford, was unanimously elected an Honorary Member of the Society.

Mr. E. W. Binney, F.R.S., F.G.S., exhibited some specimens which Mr. Samuel Ogden had procured from a bore hole now being made near the new hotel in Birkdale Park, Southport, for the purpose of obtaining a supply of water. At 65 yards deep from the surface beds of red and light coloured marls, with crystals of sulphate of lime and white granular gypsum, were met with. At 67 yards similar beds, with greater quantities of sulphate of lime and gypsum in them. At 69 yards like beds, without sulphate of lime and gypsum. All the deposits contained common salt, which was very evident on the application of the tongue. He said that red and variegated marls containing gypsum similar to those exhibited were met with in the upper permian beds of Lancashire and the keuper marls of the trias in Cheshire, but in the latter alone had salt been yet found, and he was therefore inclined to consider the specimens triassic, and the strata not very likely to yield an abundant supply of fresh water. The thickness of these marls had never been thoroughly proved, but at Fleetwood they had been penetrated 409 feet, at Poulton Breck 537 feet, without reaching the underlying marls and waterstones. At Middlesborough in the east of England, and Duncrue near Belfast, these beds

were proved to be about 1200 feet in thickness. Rock salt and not fresh water has generally been found in them. If the sandstone beds of the waterstones or the soft red sandstone of the bunter, both of them water bearing strata, were reached, he doubted about the water being fresh if found at all.

Mr. W. W. Platt, of the firm of Mather and Platt, said he was of opinion that the occurrence of salt in the marls was no reason why there should not be fresh water below in beds of sandstone, and if he were the parties engaged in the undertaking he should not hesitate in proceeding further, and he thought with a fair chance of success, for a good supply of fresh water.

"On the Manufacture of Sulphide of Ammonium," by Peter Spence, F.S.A., F.C.S.

Hydro-sulphuret of ammonia, or sulphide of ammonium, is at present chiefly used in the laboratory, where it is a very useful reagent in metallic and other analyses, and it has often been thought it might be more extensively used if it could be made cheaply and of good quality. At present it is made by directly charging liquor ammonia with sulphuretted hydrogen gas, but this is a very expensive mode, and, from the fact that most of it thus obtained is not fully charged, it would seem not to be an easy mode of making it. Having occasion some time ago to require it largely in the manufacture of sulpho-cyanide of ammonium, I invented for myself a mode of making it very economically and fully charged with sulphur.

In communicating this plan to the Society I do it in the hope that it may be useful in the laboratories of those who produce some of their own reagents. The Society must not, however, give me too much credit for generosity, for, had I found a market for the article making it worth my while to make it largely, I should have held it secured by patent,

but as it is not, I make the gift for what it is worth. I have by this plan made the article to some extent for a London house, and have sent them some 30 to 40 carboys; but the demand was only fitful, and although they were highly satisfied with the quality and would have given me my own price, it was not worth my while to continue the process.

The mode of manufacture is merely to mix a salt of ammonia, say sulphate or chloride, with double its weight of soda waste or gas lime (either being suitable), and blowing into the mixture a jet of steam and passing the vapours through condensing apparatus; the distillate is pure sulphide of ammonium. It may be as well to mix the two bodies in a portion of water; and even in that case it is needful to be careful with the condensing pipes, as sulphide of ammonium being much more volatile than water, it comes over so strong at first as to condense in a solid form. I had nearly a serious accident with it, which I name to induce caution. I was passing the still when at work, and saw something was wrong. I called my manager, who, with the man at work, examined the pipes, and found them closed; on opening them with an iron rod there was a flow of liquor and a strong blow of gas; seeing and feeling this, I called to the man to leave it instantly, but he hesitated. My manager then seized him by the arm and pulled him away, and he instantly fell down and became rigid as a log of wood; his breast was now rubbed forcibly to bring on breathing, which had quite stopped, and a doctor who was sent for poured cold water on his head. He now went into most violent convulsions, which lasted over an hour and a half, when he got gradually round and was able to work next day. I ascribed the saving of his life to the rubbing of his breast, as complete asphyxia had taken place with the rigidity of death. Sulphuretted hydrogen is most powerful in its effects on animal life, but when combined with ammonia it seems even more active;

one peculiarity of it in both forms, however, seems to be that if you escape with life it leaves no bad effects afterwards. I have myself oftener than once suffered from it, but only temporarily.

With this caution to those who attempt making sulphide ammonium, and stating that otherwise there is no difficulty in the process, I conclude this communication.

MICROSCOPICAL AND NATURAL HISTORY SECTIONS.

October 8th, 1866.

A. G. LATHAM, Esq., President of the Section, in the Chair.

MR. HURST read a paper "On the plants springing up spontaneously on the fresh turning up of pasture land at Knutsford, Cheshire."

He said that grass seeds were sown over a portion, but that this merely accelerated what appeared to be the certain effect of time in this district, viz., the almost total extinction of other plants by the grasses. His observations only extended through July and August, during which time he observed the following species:—

Ranunculaceæ.
Ranunculus bulbosus
auricomus

Papaveraceæ.
Papaver dubium, Lamottii
argemone

Fumariaceæ.
Fumaria confusa, E. B.
officinalis

Cruciferæ.
Capsella Bursa pastoris
Brassica Rapa, E. B.
Arabis Thaliana

Violaceæ.
Viola arvensis
eu-tricolor

Caryophillaceæ.
Silene arvensis, E. B.
Spergula arvensis, E. B. var. sativa
Stellaria media

Geraniaceæ.
Geranium pusillum

Leguminoseæ.
Trifolium repens
pratense
procumbens
minus
Vicia hirsuta

Rosaceæ.
Rubus
Cratægus

Rubiaceæ.
Sherardia Arvensis

Compositæ.
Achillea millefolium
Senecio vulgaris
Sonchus oleraceus
Crepis virens
Leontodon autumnalis, var. eu genuinis
Taraxacum officinalis

Borragineæ.
Myosotis arvensis

Primulaceæ.
Anagallis arvensis

Scrophularineæ.
Digitalis purpurea
Veronica serpyllifolia

Labiatæ.
Prunella vulgaris
Galeopsis versicolor
Lamium purpureum

Plantagineæ.
Plantago lanceolata
major

Chenopodiaceæ.
Chenopodium album

Polygonaceæ.
Rumex acetosa
acetosella
crispus
Polygonum convolvulus
aviculare
persicaria
lapathifolium

Euphorbiaceæ.
Euphorbia helioscopia

Amentaceæ.
Quercus pedunculata

Juncaceæ.
Juncus bufonius

Gramineæ.
Anthoxanthemum odoratum
Holcus mollis
lanatus
Lolium perenne
Bromus arvensis
Poa trivialis
procumbens
Dactylus glomeratus
Avena sativa

With regard to the foregoing list, he drew attention to the total absence of Umbelliferæ. Also the Polygonum Convolvulus was the plant which at one time covered more ground than any other species, appearing to smother most of the plants around it. Yet in untouched adjacent fields not an individual could be found. Classed by De Candolle as one of

those plants cultivated by man against his will in western Europe, and only indigenous in Siberia, it certainly seemed curious that a Siberian plant should be the first to cover the soil of Cheshire on its being first exposed to the air. Mr. Hurst remarked that Polygonum aviculare came after the former species in the extent of ground covered, and on flat unshaded places appeared to hold its own against P. convolvulus better than any other plant; nor does it appear to be eventually smothered by the grasses among which it may frequently be found growing. Papaver dubium came next as to conspicuousness, though perhaps not covering so great an area as other species humbler in appearance. In the adjacent town of Bowdon, under similar circumstances, it sprang up abundantly the first year, sparingly the second, and almost disappeared the third. The slightest cakeing or hardening of the surface soil being fatal to its existence.

Plantago major, or "the footsteps of the white man," as it is called by the North American Indians, also appeared, but in small numbers. The list comprises 58 species, of which the Graminaceæ form 17 per cent; Polygonaceæ, 13 per cent; Compositæ, 11 per cent; Leguminosæ, 9 per cent.

Hence there were one-half of the species comprised in four families. Papaver argemone, Myosotis arvensis, Anagallis arvensis were single specimens, but these species are found in the adjacent fields.

PHOTOGRAPHICAL SECTION.

November 1st, 1866.

Dr. J. P. Joule, F.R.S., &c., Vice-President of the Section, in the Chair.

Mr. Coote exhibited an interesting set of prints, including views of Exeter Cathedral, Torquay, &c., which he had recently taken by the collodio-albumen process. One of the plates used had been prepared twelve months, and the print from it was equal to most of the others taken on freshly prepared plates.

Mr. Brothers suggested that if the meteoric shower on the night of the 12th–13th instant should prove to be one of more than ordinary interest, it might be interesting to preserve a photographic record of the event. He said that a camera with a prepared collodion plate might be adjusted on some well known star near where the meteors were noticed to be most numerous and brightest, and after about ten or fifteen minutes the plate (it might be kept wet about that time) should be developed if any meteors had been observed to pass. A succession of plates could be exposed, and a note taken of the principal star to which the camera had been directed.

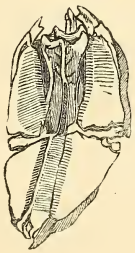
Ordinary Meeting, November 13th, 1866.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Arthur Mc.Dougall, William Stanley Jevons, M.A., Professor of Logic, &c., Owens College; and William Jack, M.A., Professor of Natural Philosophy, Owens College, were elected Ordinary Members of the Society.

"On Echinus lividus, illustrated by specimens from Roundstone," by THOMAS ALCOCK, M.D.

The Author described particularly the mechanism of the teeth and jaws of the animal, and showed by a dissection of the parts that the statement made both by Professor Owen and Professor Rymer Jones that the striated surfaces of the jaws are used to comminute the food is incorrect, for the whole of these surfaces is occupied by muscle, and is altogether outside the pharynx through which the food passes. These particulars may be seen in the accompanying figure taken from the dissection.



He further showed that the food contained in the alimentary canal consists of very coarse pieces of sea-weed and zoophyte, which have evidently not been subjected to the action of any triturating apparatus.

He exhibited mounted specimens of the suckers, and also of the sucker-plates cleaned in potash. He said Professor Owen quotes Professor Valentin with regard to the pedicellariæ, and states that there are three forms of them belonging to Echinus lividus, namely, gemmiform, tridactyle, and ophicephalous pedicellariæ;—these were exhibited as mounted specimens, and with them a fourth kind, quite distinct from them, and the most remarkable in form; it has long slender jaws like those of a crocodile, armed, in this species, with one very long terminal tooth and one tooth on each side not far removed from it. He remarked that in Echinus sphæra all four kinds of pedicellariæ are found and agree in their general character with those of Echinus lividus, though they are sufficiently different to be readily distinguished, and the fourth kind just mentioned has, besides the long terminal tooth, a series on each side, of six or seven recurved teeth, suggesting that the name sauricephalous would be an appropriate one for this form. Mounted specimens of the four kinds of pedicellariæ of Echinus sphæra were shown for comparison with those of Echinus lividus, together with suckers and sucker-plates, and the buccal membrane mounted entire to show the ophicephalous and gemmiform pedicellariæ complete and in their natural position.

"On the Structure of the spines of Echini," by H. A. Hurst, Esq.

Notwithstanding the general appreciation by microscopists of the spines of Echini, the author had been unable to find any satisfactory account of their structure, and he attributed this to the fact that the examination of these objects had been chiefly confined to their sections, mounted in Canada balsam, which frequently has the effect of making transparent objects too transparent. He recommended for this purpose, however, the use of Smith and Beck's semi-paraboloid lieberkhun, together with transmitted light — cutting off either means of illumination by a slight motion of the hand, or using both together; but the readiest means of ascertaining their real structure he found to be the examination of unmounted and splintered ends of broken spines by incident light, a method bringing out details and showing the connection of parts in a manner superior to any other. It was with diffidence he dissented from Dr. Carpenter's views in the last edition of his work on the microscope; but he begged to propose the following as more in accordance with the appearances of structure presented by these spines under the microscope. They are composed of two substances in outward appearance, though chemically perhaps the same, one so perfectly homogeneous and transparent when viewed by transmitted or polarised light that it cannot be distinguished from the blank field of the microscope — yet, under incident light, so dark and opaque as to appear black. This substance is frequently traversed by winding anastomising channels, which, though only containing air, seem opaque and show as solid by transmitted light, the substance they

traverse itself being invisible. He had not satisfactorily made out the structure of the second substance; it resembled the pith of plants, but it was less regularly cellular, and in some spines assumed a fibrous appearance. It is opaque under transmitted, and glistening white under incident light. In the following remarks he called this opaque, and the first described transparent substance. The general structure of the spines he had examined was also twofold; in the simpler, as the Amphidotus cordatus, the centre portion is hollow; in the more complicated it is composed of the opaque substance perforated along the length of the spine by vertical solid tubes of the transparent matter, without any definite arrangement. These appear to increase only in length; hence a section at the apex of the spine shows in the centre a prolongation of the oldest portion, the thickening of the spine arising differently, as subsequently explained. The hollow centre of the Amphidotus cordatus is surrounded by a cellular fretwork of the transparent matter, while around this is a circle of solid ribs or pillars of the same, smooth on the exterior of the spine, but within beautifully hollowed out into what the heralds call an engrailed outline, the points of which connect it with the inner layer of cellular fretwork. This framework is occasionally wanting, and the engrailed points are simply connected with each other by a straight inner line of transparent matter. In the more complicated forms he was not satisfied he had ascertained the real structure, but thought it to be as follows.—The tubes of transparent matter noticed about the central opaque substance, as they approached towards the circumference of the first season's growth, gradually coalesce, and,

at a certain distance from the centre, consolidate into a rib or pillar, which runs from the root to the apex of the spine, forming a longitudinally furrowed exterior, caused by the centre of each rib projecting slightly. Once this consolidation of tubes into ribs effected, the growth changes, the ribs extending eccentrically into plates radiating from the centre, and separated from each other by a mass of tissue similar to that at the centre. At the close of the second period of growth these plates thicken concentrically so as almost to touch each other, which however they do not, leaving a furrow of separation. They then continue their radiating extension till the end of another year's growth, when the concentric thickening into an outer rib again takes place, and so on in successive years. These radiating plates are horizontally perforated by circular apertures bearing a singular resemblance to rivet holes in boiler plates, and appear to have rivets passing through them of the opaque substance in a fibrous state. Altogether the structure of the spine may be compared to that of an iron tubular bridge. The thickened exteriors of these plates are highly coloured, and traversed by winding, anastomising channels containing air; in some portions of the spine, the opaque substance either grows into, or is destroyed and replaced by, the transparent substance, which then forms a solid mass, perforated by the rivet holes, now changed into winding, anastomising passages.

Forms intermediate between the extremes of complexity and simplicity were those of Echinus sphæra and lividus.

Echinus sphæra being chiefly composed of the pith-like substance, with 25 or 30 radiating glassy plates of a whitish

colour; while

Echinus lividus was more solid, the pith-like substance passing into the solid glassy radiating plates through portions consisting of this glassy matter, perforated by anastomising channels.

Mr. Hurst was not able to say whether the pithy and glassy substances are distinct or not; but while the cellular matter leaves no trace after the prolonged action of vinegar, the transparent glassy substance, as well as the exterior of the spine, appears to be enveloped by a membrane, resisting the action of vinegar, which curiously converts this solid, opaque, hard, and brittle spine into a transparent, flexible body, retaining its original form.

By using direct sunlight and a semi-paraboloid condenser, the glassy matter could be distinctly seen through, even when viewed as an opaque object, and the arrangement of the cellular matter ascertained. It is this transparent substance which is tinged with the beautiful purple hue so well known to microscopists.

Mr. Hurst expressed his disappointment that the use of polarized light in these observations had led to no result, and thought its value had been over-estimated.

Ordinary Meeting, November 27th, 1866.

E. W. Binney, F.R.S., F.G.S., Vice-President, in the Chair.

Among the donations announced was a beautiful series of preparations of the spores of Fungi on glass, from Sir J. F. W. Herschel, Bart., M.A., F.R.S., &c. &c.

On the motion of Mr. Spence, seconded by Dr. Joule, the thanks of the Society were unanimously voted to Sir John Herschel for his valuable donation.

"Observations of the Meteoric Shower of November 13-14, 1866," by Joseph Baxendell, F.R.A.S.

The early part of the night of November 13th was very squally and cloudy, with showers of rain and hail and occasional flashes of lightning. At about 12h. 15m. a break occurred near the zenith, and in a few minutes the clouds had almost entirely disappeared. My observations of the meteors commenced at 12h. 16m. Greenwich mean time, and were directed principally to the determination of the time of maximum frequency, and the position of the radiant point. The observations of frequency were as follows: —

	Number of Meteors Observed.
From $12^h\ 16^m$ to $12^h\ 32^m$	60
32 — 48	153
48 — 13 4	287
13 4 — 20	378
20 — 26	122
26 — 42	316
14 19 — 14 42	54
15 20 — 15 35	6

From 13h. 42m. to 14h. 19m., and again from 14h. 42m. to 15h. 20m., the observations were interrupted by clouds and rain, and only 73 meteors were counted during the two

intervals. At 15h. 35m. clouds came on again very suddenly, and the sky remained obscured at 16h. 5m., when I ceased to watch.

During the whole time of observation the sky was rarely entirely free from clouds for more than two or three minutes, but the errors arising from this cause are probably pretty evenly distributed through the intervals above given, and cannot therefore materially affect the final determination of the time of maximum frequency. The results of the observations are as follows:—

	Average Number of Meteors per Minute.
At 12h 24m	3·7
40	9·5
56	17·9
13 12	23·6
23	20·3
34	19·7
14 30½	2·3
15 27½	0·4

The curve formed by a projection of these numbers gives 13h. 12m. as the time of maximum frequency. The probable error of this result can hardly exceed one minute.

In order to determine the position of the radiant point, the positions of the intersecting points of the paths, continued backwards, of a great number of pairs of meteors were noted. By far the greater number of these points fell on a space bounded by lines joining the stars γ, ζ, μ, ϵ, and η Leonis, and, allowing equal weights to all the observations, the mean position was found to be R. A. 9h. 58m. 12s. $=149°\ 33'$; Dec. $22°\ 57{\cdot}5'$ North. Calculating the position referred to the ecliptic, we have Long.$=143°\ 41{\cdot}0'$; Lat.$=9°\ 54{\cdot}5'$ North.

At the time of maximum frequency the earth was advancing in the direction of a point on the ecliptic the longitude of which was $141°\ 28{\cdot}3'$, or $2°\ 12{\cdot}7'$ less than that of the radiant. It appears, therefore, that the meteors were

crossing the earth's orbit from within outwards, and that their aphelion distance is very sensibly greater than the earth's radius vector on the 13th of November.

The velocity of the earth in its orbit on the 13th of November is 18·38 miles per second, and the velocity of the November meteors when they enter the earth's atmosphere has been found to be 40 miles per second. With these data and the latitude of the radiant point as given above, 9° 54·5′ N., we find that the inclination of the orbit of the mass of meteors to the plane of the ecliptic is 17° 59′, and that their orbital velocity at the time they encounter the earth is 22·31 miles per second. The excess of this velocity over that due to their distance from the sun arises, in part at least, from the accelerating effect of the earth's attraction.

An attempt was made to estimate roughly the relative numbers of meteors of different magnitudes, and it was found that they occurred in about the following proportions: —

Out of every 100 meteors, 10 were above the 1st mag.; the brightest of these were 2 to 3 times brighter than Sirius;
15 were between the 1st and 2nd mag.
25 „ 2nd and 3rd mag.
30 „ 3rd and 4th mag.
15 „ 4th and 5th mag.
5 were below the 5th mag.

The average magnitude was 3·0.

The trains left by many of the larger meteors had a beautiful emerald green colour, others were of an ashy grey, and the remainder white. The meteors themselves were mostly white or bluish white, but many were of a fine golden colour.

In order to give some idea of the great velocity with which the meteors enter the earth's atmosphere it may be remarked that it would be sufficient to carry a body through the entire circuit of the earth in an interval of less than ten and a half minutes.

As I had the good fortune to witness the great meteoric shower which occurred on the morning of the 13th Nov., 1833, I may state that the late display was far inferior to it both in the number of meteors seen and in the brilliancy of the larger ones, and I am therefore inclined to think that a much finer display may be expected to occur in November next. At the time of the 1833 great shower I was at sea, off the west coast of central America, and although I then knew little about meteors, and the idea of a radiant point had not, so far as I am aware, ever occurred to any astronomer or meteorologist, the tendency of the great majority of the meteors to diverge from a particular region of the heavens was so strongly marked that it at once engaged my attention, and I find on referring to my notes that I fixed the central point of this region in the constellation Cancer, a few degrees east of the stars δ and γ, and not in Leo, as observed by Professor Olmstead and others, in the north-western portion of the North American continent. A great number of the meteors, however, had other radiant points, and some of the finest moved in long horizontal arcs, or in directions nearly perpendicular to that of the main stream. This fact seems to me to be strongly opposed to the cosmical theory of meteorites, except on the rather improbable supposition that the earth, on that occasion, encountered two or more groups, all, at the same time, crossing each others orbits, as well as the orbit of the earth. It may, however, be urged that such a supposition is hardly more unlikely than that which ascribes the November meteors to a ring of small bodies moving round the sun in an orbit differing little in magnitude from the earth's orbit, but the motion being retrograde, or contrary to that of the earth, and therefore inconsistent with the general analogies of the solar system, and opposed to Laplace's almost universally received nebular hypothesis.

"Observations of the new variable star, T Coronæ," by Joseph Baxendell, F.R.A.S.

While engaged on the night of the 15th of May last in observing some of the naked eye variables, my attention was suddenly arrested by a strange and rather conspicuous star about a degree distant from ε Coronæ in the south following quadrant. It was at once carefully compared with some of the neighbouring stars in order to ascertain its magnitude exactly, and a rough determination of its position was made, which led me to conclude that it was identical with Argelander's No. 2,765 of Zone + 26°, 9·5 magnitude, of the *Bonner Sternverzeichniss*, a conclusion which was fully confirmed the following night by more exact observations. On the night of the 7th of May I observed all the naked eye variables then visible, and also several telescopic ones, and among the latter R and S Coronæ, but this star, if at that time really visible, entirely escaped my notice; and as the nights between the 7th and 15th were cloudy at Manchester, I am unable to say at what date it attained its maximum brilliancy. My observations of its magnitude, colour, &c., from the date of discovery to the present time are as follows, and I may add that all determinations of magnitude after the star became invisible to the naked eye were, with only three exceptions, made at Mr. Worthington's observatory with his equatorially mounted achromatic of five inches aperture, care being taken to use always the same eyepiece, a positive, having a magnifying power of 68 times.

Observations of T Coronæ.

Date.	Mag	Colour, &c..
1866.		
May 15	3·7	White, with a very slight yellow tinge, whiter than ε.
16	4·2	Cream coloured, but the light very bright, and star well defined, without any hazy appearance.
17	4·9	With naked eye. Cream-coloured, exactly similar to ε.
	5·1	With 5in. Ach. p. 68.

Date.	Mag	Colour, &c.
1866.		
May 18	5·3	Cream-coloured or buff. At times I have an impression of a blue tinge, as if the yellow of the star were seen through a film of a blue tint.
19	5·6	Deep cream, buff, Bath brick or wash-leather colour, with a tinge of blue over it. ϵ the same colour, but perhaps slightly lighter and without the blue, or at least with excessively little of it. Repeatedly examined also by Mr. Dancer and Mr. Williamson, with different powers, and their estimations of colour precisely the same as my own.
20	6·2	Buff coloured, with a tinge of blue, and deeper than ϵ Coronæ, which is yellow or light buff.
21	7·1	Leaden or slaty blue; the yellow colour has almost entirely disappeared.
22	7·4	The light of the star is dull, and is of a slaty blue or dark French white colour, or nearly like Smyth's No. 4 blue. No traces of yellow or red can be clearly made out.
23	7·5	Dull grey or French white. Sometimes there seems to be a trace of yellow.
24	7·7	Dull white, with a slight tinge of yellow or orange.
25	7·8	Dull and slightly orange-white. A shade of blue sometimes suspected.
26	8·0	Dull orange white.
29	8·4	Dull orange yellow.
June 8	9·3	Dull orange yellow.
10	9·2	Dull orange yellow.
16	9·4	
17	9·5	
19	9·5	
25	9·6	Orange yellow.
26	9·7	Orange.
July 11	9·7	Dull yellow.
16	9·7	
19	9·6	
20	9·7	
21	9·6	
22	9·5	Dull pale orange.
30	9·7	Orange yellow.
Aug. 20	9·8	
27	9·5	
31	9·3	Dull yellow.
Sept. 14	7·9	Dull yellow; almost exactly Smyth's yellow No. 3.
15	7·8	Yellow.
17	7·9	Pretty bright yellow.
22	7·9	
24	7·8	Yellow.
30	7·8	
Oct. 1	7·7	
6	7·6	Greyish yellow.
8	7·6	
10	7·5	Yellow.
14	7·5	Light yellow.
19	7·7	Light yellow.
28	7·8	Yellow.
Nov. 6	7·9	Smyth's orange No. 4.
19	8·3	Dull ruddy orange.

It will be seen that the brightness of the star diminished with great rapidity for several days after my first observation, and afterwards more gradually, and that on the 26th of June it had sunk to the 9·7 magnitude. It then remained with little change till about the 20th of August, when another rise commenced, and on the 15th of September it had attained the 7·8 magnitude. On the 10th and 14th of October it was of the 7·5 magnitude, and since the latter date its brightness has again slightly diminished.

It will also be noticed that in the recent observations no mention is made of the blue tinge which formed so striking a feature in the colour of the star for some time after its first appearance. In connection with this the following extract of a letter from Mr. Huggins, F.R.S., dated October 13th, will be interesting to the members of the Society. Referring to T Coronæ the writer says, "I observed its spectrum on September 16th, 27th, 28th, and October 8th. The bright lines are not now to be distinguished. If they exist they cannot be much, if any, brighter than the parts of the spectrum where they occur. The observation of its spectrum is now very difficult."

In the scale of magnitudes I have employed, the light-ratio is 2·512 ; but the expression in magnitudes of the brightness of a variable star gives a very imperfect idea of the nature and extent of its changes, and as any speculations respecting the causes of variability must be based upon a consideration of the variations in the intensity of the light, and not in the magnitude of the star, I have calculated the relative intensities of the light of T Coronæ for every night of observation, taking the intensity of the light of a 10th magnitude star as unity. The results are given in the following table :

Intensities of the Light of T Coronæ :—

Date.	Mag.	Intensity, 10 mag. being=1·0	Date.	Mag.	Intensity.	Date.	Mag.	Intensity.
1866.								
May 15	3·7	331·2	June 17	9·5	1·6	Sep. 17	7·9	6·9
16	4·2	209·0	19	9·5	1·6	22	7·9	6·9
17	4·9	160·0	25	9·6	1·4	24	7·8	7·6
18	5·3	75·8	26	9·7	1·3	30	7·8	7·6
19	5·6	57·5	July 11	9·7	1·3	Oct. 1	7·7	8·3
20	6·2	33·1	16	9·7	1·3	6	7·6	9·1
21	7·1	14·4	19	9·6	1·4	8	7·6	9·1
22	7·4	11·0	20	9·7	1·3	10	7·5	10·0
23	7·5	10·0	21	9·6	1·4	14	7·5	10·0
24	7·7	8·3	22	9·5	1·6	19	7·7	8·3
25	7·8	7·6	30	9·7	1·3	28	7·8	7·6
26	8·0	6·3	Aug. 20	9·8	1·2	Nov. 6	7·9	6·9
29	8·4	4·4	27	9·5	1·6	19	8·3	4·8
June 8	9·3	1·9	31	9·3	1·9			
10	9·2	2·1	Sep. 14	7·9	6·9			
16	9·4	1·7	15	7·8	7·6			

The outburst of T Coronæ appears to have been first observed by Mr. J. Birmingham, of Tuam, on the night of May 12, when its brightness was equal to, if not greater than that of α Coronæ, the magnitude of which is 2·6. The relative intensity of the light of the variable on that night was therefore not less than 912·1. Comparing this with the intensities in the above table it will be seen that during the first ten days of the star's appearance its light diminished with extraordinary rapidity, the intensity on the 22nd May being only 11·0 against 912·1 on the 12th when first seen by Mr. Birmingham, and 331·2 on the 15th when first seen by myself. Since the 22nd of May its changes have been comparatively slight. On the 20th of August the intensity was at a minimum, being only 1·2, or $\frac{1}{760}$th of its amount on the 12th May.

An inspection of the curve of intensities suggests strongly the idea that a force of an explosive character, such as could result only from the action of highly elastic gaseous matter, had been in operation to produce the sudden increase and subsequent rapid diminution of brightness which had

taken place; but as some of the well-known periodical variables occasionally exhibit an equal, and even a greater, rapidity of change, this view cannot at present be received with much confidence; and notwithstanding the remarkable and highly interesting conclusions which Mr. Huggins and Dr. W. A. Miller have drawn from the results of their spectroscopic observations of this new variable, we are constrained to admit that the cause of variability is still involved in the deepest mystery.

The following table contains the positions and magnitudes of the stars with which I have compared T Coronæ in the course of its changes. The positions are from the "Bonner Sternverzeichniss," and the magnitudes are from my own observations. The magnitudes of some of these stars have been determined independently by Mr. Knott, F.R.A.S., and it is satisfactory to me to find that our results are almost identical.

No.	Star.	R. A. 1855·0.	Dec. N. 1855·0	Magnitude.
1	β Serpentis.....	$15^h\ 39^m\ 29{\cdot}3^s$	15° 53·1′	3·7
2	γ Herculis	16 15 31·1	19 30·3	3·8
3	γ Coronæ	15 36 40·0	26 45·6	4·2
4	ϵ Coronæ	51 35·7	27 18·1	4·3
5	δ Coronæ	43 31·0	26 31·5	4·8
6	π Serpentis.....	56 3·5	23 12·3	4·8
7	ι Coronæ.....	55 38·2	30 15·1	5·1
8	25 Herculis.....	16 20 14·8	37 42·7	6·0
9	Arg. 2575 + 27°	15 56 38·2	27 1·1	7·5
10	3009 25	53 23·8	25 51·1	7·6
11	2767 26	55 2·8	26 34·6	7·7
12	2762 26	52 32·8	57·2	7·9
13	2754 26	49 16·8	26·3	8·0
14	3003 25	52 39·3	25 59·5	8·1
15	2563 27	52 30·0	27 16·8	8·1
16	2769 26	55 28·9	26 48·0	8·4
17	2763 26	52 54·1	33·6	9·0
18	2758 26	51 49·5	9·9	9·2
19	2760 26	52 7·3	40·0	9·4
20	2761 26	52 25·4	21·2	9·6
21	2764 26	53 22·3	33·9	10·8

I may state that two of these stars, Nos. 10 and 18, have shown decided indications of slight variability, the range of variation, so far as I have yet observed them, being about four-tenths of a magnitude.

Ordinary Meeting, December 11th, 1866.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On the Recent Suspension, by the Board of Trade, of Cautionary Storm Warnings," by JOSEPH BAXENDELL, F.R.A.S.

On the 29th ult. a circular was issued by the Board of Trade, announcing that the "cautionary storm warnings," which have from time to time been issued by the Meteorological Department of the Board, would be suspended from the 7th day of December instant. This step, it appears, has been taken on the recommendation of the President and Council of the Royal Society, who are of opinion that "at present these warnings are founded on rules mainly empirical, and therefore should not be issued under the superintendence of the scientific body to whom the discussion of meteorological observations will be committed." No attempt appears to have been made by the Board to ascertain the opinions and wishes of those classes of the community for whose benefit the system of issuing storm warnings was originated by the late Admiral Fitzroy. No complaints against the system, as carried out by the admiral, and since his death by Mr. Babington, have been made by those most deeply interested in its operation; nor has any evidence been produced to show that it has led to loss or inconvenience. On the contrary it is very generally admitted that it has been the means of saving many valuable lives, and also an amount of property greatly exceeding in value the whole

cost of the department in which it had its origin. Whether the rules on which it is founded are empirical or not, the system, as a mere commercial speculation, has been successful, and it is therefore difficult to understand why the Board of Trade should have attached so much importance to the reasons advanced by the President and Council of the Royal Society, and allowed them to outweigh other far more important considerations. If the President and Council think that the superintendence of the duties connected with the system of issuing storm warnings would be derogatory to the scientific body to whom the discussion of meteorological observations will be committed, it does not follow that the system ought to be suspended for an indefinite length of time, but rather, that its management ought to be committed to some more suitably constituted body. If, however, it is intended to be understood that the system ought to be discontinued because the rules upon which storm warnings are founded are, at present, mainly empirical, then it may be replied that, in the early stages of every science, its rules are mainly empirical; but it has never before been urged that, therefore, we ought to deprive ourselves of such advantages as may be derived from the limited amount of knowledge we may happen to possess. The rules by which the laws of magnetism are applied to the correction of compasses in iron ships are to a great extent empirical, and, in spite of the greatest care in their application, serious errors are sometimes made; but are we on that account to decline to avail ourselves of such aid as they do afford, imperfect, and to some extent uncertain, though it may be? Long after the predicted places of the moon were given in the *Nautical Almanac*, the longitudes derived from lunar distances were often very considerably in error; but no scientific man, or body of men, at that time ventured to suggest that the publication of the *Nautical Almanac* ought to be suspended until astronomers had

perfected the lunar theory. The laws relating to the strength of materials were long in a very unsatisfactory state; but scientific men did not urge that, therefore, we ought to suspend building and engineering operations until these laws had been established on a strictly scientific basis. It is needless, however, to multiply illustrations to show the unsoundness of the principle on which the recommendation is based; and it requires but a moderate acquaintance with scientific methods to see that in a science which aims at prediction, its progress will be best and most surely advanced by a continuous and systematic comparison of the phenomena as they actually occur with the phenomena as predicted. It is in this way that the methods of astronomy have attained their present high degree of perfection; and it will be obvious, on a little consideration, that, if Mr. Babington's predictions or forecasts of the weather are regulary and systematically compared with the phenomena which actually occur, many points of considerable interest will doubtless often be brought under immediate notice which might otherwise long escape detection, and in this way an impetus will be given to the progress of the science which could not be applied in any other way, or by any other means.

In the circular issued by the Board of Trade it is stated that the President and Council of the Royal Society think that "in a few years the rules on which storm warnings are founded may probably be much improved by deductions from the observations in land meteorology, which will by that time have been collected and studied." Now, this allusion to land meteorology refers, no doubt, to a recommendation made in a letter addressed by the President of the Royal Society to Mr. Farrer, one of the Secretaries of the Board of Trade, on the 15th June, 1865. Mr. Farrer had informed the President "that the Admiralty were willing to undertake, and to place in the hands of their Hydro-

grapher, all those observations which can properly be made use of in framing charts for purposes of navigation, but not those which relate to meteorology proper;" and the President, in referring to this, interprets the term "meteorology proper" to mean merely "the land meteorology of the British Islands;" but how this singular interpretation was arrived at is not stated. He then goes on to urge the desirability of establishing "a few stations—say six—distributed at nearly equal distances, in a meridional direction, from the South of England to the North of Scotland, furnished with self-recording instruments, supplied from, and duly verified at, one of the stations, regarded as a central station." This central station, he afterwards suggests, should be the Observatory of the British Association, at Kew. Now, it has long been known to meteorologists that deductions from observations made within the narrow limits of the British Islands are quite insufficient to lead to the detection and development of the laws which regulate those great movements and phenomena of the atmosphere upon which the daily changes in the weather at any given station depend. The principal meteorological elements of the British Islands have already been determined with considerable accuracy, from observations made during long series of years, at various observatories and stations, both public and private; and any minute changes in their values that may be rendered necessary by future observations with improved instruments, are not at all likely to have any sensible influence on the empirical laws upon which storm warnings have been based. The truth is, this land meteorology scheme is altogether unnecessary for the purpose of correcting these empirical laws. It is not so much in the multiplication of observations over limited areas, as in the adoption of improved methods of combining and treating observations at stations scattered some distance apart over every accessible portion of the earth's surface, that we must

look for new discoveries, and for the means of establishing the rules of meteorology on a strictly scientific basis.

The cost of carrying out the land meteorology scheme would be out of all proportion to the results ever likely to be obtained from it; and it would be unjust to the commercial community to withdraw the funds which have hitherto been so usefully applied in the interests of humanity and commerce, and devote them to the establishment, by an irresponsible body, of a scheme of such doubtful utility.

It will be evident from the above remarks that the recommendation of the President and Council of the Royal Society, from whatever point of view it may be regarded, is a retrograde movement, opposed to the true interests of the science of meteorology, and likely, if acted upon, to retard its progress. It is therefore to be hoped that the Board of Trade will reconsider their decision, and not, for light and insufficient reasons, discontinue a system which, on grounds of humanity and commercial economy, has met with the general approval of the country, and is, moreover, so likely to contribute materially to the advancement of a popular and highly-important branch of science.

Mr. Thos. Heelis, F.R.A.S., urged that the abolition of the cautionary signals would tell hardly against the coasting craft and fishermen. To large well-found sea-going vessels, well provided with instruments, the signals were not of much use, but the case was very different with the masters and owners of the coasting vessels who were unable to afford instruments. He instanced the case of the colliers which supply the large steamers at Southampton with coal brought from the south Welsh ports. These vessels have to make all their passages from Southampton to the westward in ballast, and it is of the utmost importance to them not to be caught by a gale from the southward between Plymouth or Falmouth and the Land's End. He was not aware that

any complaints as to the inefficiency of the storm warnings had been received from the masters of coasters, and considering the amount of capital and number of hands employed in the trade, and the fact that they had no powerful organ to advocate their views, he thought that their interests deserved consideration, and feared that they had been overlooked. The saving of life should, especially at present when so many complaints are current about the deficiency in the supply of seamen, be a great object, even if such saving were effected by empirical means. Mr. Heelis had no confidence in the prospect of improving the meteorology of the coast by observations inland. He rather advocated the improvement of the land meteorology by observations on the coast; and he had noticed a decided improvement of late in the storm warnings. Having been engaged in a yachting cruise on the Cornish coast during the months of August and September last, he had paid a good deal of attention to the signals, and had never seen them hoisted without the forecast being speedily verified.

Mr. Binney, F.R.S., stated that Professor H. R. Goeppert, the renowned German botanist, had lately described a fossil plant under the name of *Aphyllostachys Jugleriana*, which he classed in the group of *Calamarien*, and stated, with some degree of doubt, that it probably belonged to the lias formation of Hanover. The specimen had been in his possession fifteen years, and he says, "In spite of the lively interest which it created on nearer examination, still its relation to other plants of the former and present world remained doubtfnl, so that I often laid it aside for time to give me further information. Since this hope has so far not been realized I do not fail in making it public at last, with the wish that others guided by deeper insight may succeed better than I have done in its explanation." The author alluded to the resemblance of the fossil to the fructi-

fication of certain coal measure plants, particularly to the *Volkmannia sessilis* of Presl. Mr. Binney said he had in his cabinet two specimens showing organs of fructification very similar to those described by Professor Goeppert, and evidently closely allied to them. These specimens were from the carboniferous strata of Great Britain, one from the mountain limestone of North Wales, and the other from the upper coal measures at Ardwick, near Manchester, so in all probability the learned Professor's specimen came from carboniferous and not liassic strata. The fossil is the fructification of some species of *calamites*, according to the opinions of the best fossil botanists of the day.

Mr. W. L. Dickinson communicated a paper containing the results of calculations relative to the Occultation of the star Aldebaran by the Moon, January 16, and to the Eclipse of the Sun, March 5, 1867, visible here. The calculations have been made for the observatory of Robert Worthington, Esq., F.R.A.S., Crumpsall, near Manchester, Lat. 53° 30′ 50·0″ N., Long. $0^h\ 8^m\ 56{\cdot}16^s$ W. The elements used in the computations have been obtained from the *Nautical Almanac*.

The Occultation of the star α Tauri (Aldebaran) by the Moon, January 16, 1867:—

	Sidereal Time at Observatory.	Mean Time at Observatory.	Mean Time at Greenwich.	Angle from N.Point.	Angle from Vertex.
Disappearance	$2^h\ 57^m\ 1^s$	$7^h\ 13^m\ 58^s$	$7^h\ 22^m\ 54^s$	100°	81°
Reappearance.	$4^h\ 10^m\ 5^s$	$8^h\ 26^m\ 50^s$	$8^h\ 35^m\ 46^s$	282°	280°

The Angles are reckoned towards the right hand round the circumference of the Moon's image as seen in an inverting telescope.

The Annular Eclipse of the Sun, March 5, 1867, is visible as a partial one at the observatory, and

Begins ..... $20^h\ 19^m\ 55^s$ mean time at Greenwich.
Greatest Phase $21^h\ 32^m\ 33^s$ „ „ „
Ends ..... $22^h\ 50^m\ 24^s$ „ „ „

Magnitude of the Eclipse (Sun's diameter=1) 0·721.

Angle, from North Pole, of { first contact, 125° towards the West. / last contact, 84° towards the East.

Angle, from Vertex, of... { first contact, 93° towards the West. / last contact, 100° towards the East.

for *direct* image.

"Notes on Varieties of Sarothamnus Scoparius, Koch., and Stachys Betonica, Benth., from the Lizard, Cornwall." By Charles Bailey, Esq.

The Lizard district has long been known to be singularly prolific in critical and rare British plants; and the purpose of this communication is to draw the attention of botanists to what appear to be two undescribed but well-marked forms of the plants whose names are placed at the head of this notice, and which are found in that district.

I.—*Sarothamnus scoparius*, Koch., var.

It is only in recent years that this plant has been admitted a Cornish species, Mr. H. C. Watson, in vol. i. of his "Cybele Britannica," p. 274, giving Devon, Isle of Wight, and Kent as its most southern limit; but in the additions included in vol. iii. of the same work, Mr. Watson states (p. 404) that "the south limit extends to Cornwall, according to Mr. Gibson and Mr. Pascoe"—no details, however, being given as to the precise part of the county in which it occurs. The specimen exhibited was found growing in small patches on the cliffs of serpentine rock about Vellan Head, situate about four miles north-west of the Lizard Lights, and it differs from the normal form, here named var. *a*, in the following characters:—

Var. *α*, *erecta*.—Stems erect, bushy; leaves stalked, the petioles as long as, or longer than, the leaflets; leaflets elliptical-obovate, bluntish.

Var. *β*, *prostrata*.—Stems prostrate, spreading; leaves shortly stalked or sessile; leaflets ovate-acute, acuminate.

The Cornish form, here named *β*, *prostrata*, differs from the normal plant chiefly in its habit of growth, which, instead of being erect and bushy, is remarkably prostrate, the branches spreading out in fan-shaped patches, and growing flat upon the ground; the branches, particularly in the upper half, are densely clothed with short spreading hairs; the leaves have shorter stalks, with a greater tendency to suppress the two lateral leaflets, the majority of the leaves, in fact, being unifoliate; the pods are less numerous, have their dorsal and ventral sutures covered with long silky hairs, and are black rather than brown, shorter, and have fewer seeds.

The season was too far advanced for any flowers to be met with, either on Vellan Head or in the small valley running down from Jollytown—the only other locality in Cornwall where the plant was observed.

II.—*Stachys Betonica*, Bentham var.

Of this plant three well-marked forms have been described: *a*, *Betonica hirta*, Reich.; *b*, *B. serstina*, Host.; and *c*, *B. stricta*, Ait.; and in many respects the form about to be described agrees with the first of these forms. In Mr. Babbington's Manual (ed. v., p. 261) it is stated that "the English plant has the round crenate, not emarginate, lower lip of *B. hirta* (R.);" but Professor Boreau is of opinion that, while the three forms just named preserve their remarkable differences of aspect when cultivated together, the distinctive characters furnished by the divisions of the corolla are but slightly constant. (*Flore du Centre de la France*, &c., ed. iii., vol. ii., p. 530.)

Stems decumbent, numerous, radiating from the root-stock, square above, rounded below, clothed with many short hairs, which are closely appressed in the upper part and pointing downwards, those in the lower part more spreading, but still much reflexed; spikes slightly inclined, just raised above the ground, compressed-globose, the verticils many-flowered, never distant; calyx covered with straight hairs, the sepals ending in stiff points; corolla three times longer than the calyx, the exterior covered with scattered shaggy hairs, which are long and silky at the base of the tube, but becoming shorter and more scattered as they approach the lip; opening of the mouth very wide, lower lip crenate, wavy; lower leaves on long stalks, cordate at the base, oblong, regularly crenate, glandular on the under surface, with short scattered hairs, upper leaves lanceolate on short stalks.

Specimens of *B. hirta,* Reich., have not come under my notice, nor have I been able to meet with Reichenbach's diagnosis; but the form described above seems to agree very nearly with Professor Boreau's description of that plant, which is here appended for the sake of comparison: "Stem clothed with many short stiff hairs; leaves with soft long hairs, very distinctly crenate; spike short, interrupted; calyx softly hairy at the summit; lower lip of the corolla rounded crenate." *(Flore, &c., loc. cit.)* Mr. Bentham in his *Labiatarum genera et species,* p. 532, gives, amongst the synonyms of his *Stachys Betonica,* "*Betonica hirta,* Leyss., Reichb., Icon., Bot. Eur. 8, 4, t. 711," which may be identical with *B. hirta,* Reich.; but the only reference to it which I have met with is in Dr. Garke's *Flora von Nord- und-Mittel Deutschland,* where it is shortly described as "Var. *a, hirta,* Leyss.— Stem with short hairs, calyx rough-haired." (Ed. vi., p. 318.)

The Cornish form is very plentiful on the cliffs of "Killas" rock, lying between Caerthilian and the Lizard Lights, grow-

ing with *Genista tinctoria*, L., var. *humifusa*, Dicks., which it much resembles in habit. The same form is also met with in several other parts of South-Western Cornwall, as at Cuddan Point and the Mount's Bay district generally.

The above communication was preceded by a few remarks on the following plants of South-Western Cornwall, specimens of which were exhibited at the meeting:—

Raphanus maritimus, Sm.....Cliffs under the Lizard Lights.
Brassica alba, L. ..... „ „ „
Arenaria verna, L., var. β, Gerardi
Willd .....Rocks at Rill Head.
Spergularia rupestris, Lebél non
Camb .....Nanjissal Bay, Land's End; plentiful.
Tamarix Anglica, Webb.....Mount's Bay.
Lavatera arborea, L. .....Cliffs, Newlyn.
Trifolium subterraneum, L. .....Penzance.
„ scabrum, L. ..... „
Anthyllis vulneraria, L. (a very
robust form) .....Porthgwarra, Land's End.
Anthyllis vulneraria, L., var. β,
Dillenii, Schult .....Forming the herbage on the sandy downs above Whitsand Bay, and common elsewhere.
Genista pilosa, L. ....Gue Graze.
„ tinctoria, L., var. β, humifusa, Dicks.....Plentiful between Caerthilian and the Lizard Lights.
Illecebrum verticillatum, L. .....Madron parish.
Herniaria glabra, L. .....Common at the Lizard.
Valerianella olitoria, Mönch .....Fields, Sennen Cove.
„ dentata, Koch ..... „ „ „
Wahlenbergia hederacea .....St. Paul, and generally distributed.
Erica vagans, L.....Gomhilly, Pradannack, and Lizard Downs.
„ ciliaris, L.....Edgecombe Downs, Carclew.

Erythræa pulchella, Fries .....Mount's Bay.
„ centaurium, Pers. .....A stunted broad-leaved form, from Porth Curnnow (non E. latifolia, Sm.)
„ littoralis, Fries .....Mount's Bay.
Sibthorpia Europæa, L.....St. Madron's Well.
Linaria Elatine, Mill.....Marazion.
Allium sibiricum, L. .....Rill Head.
Asparagus officinalis, L. (?) ..... „
Asplenium lanceolatum, Huds ...Whitsand Bay.

"Notes on Wood-Feeding Coleoptera," by JOSEPH SIDEBOTHAM, Esq.

The number of species of coleoptera that feed upon wood in this country is considerable, some attacking growing trees, others when cut down or partially decayed, others attack solid timber when cut up and used for buildings or furniture. The various species are not confined to one or two of the great divisions, but are to be found scattered through most of them, being found in the sections *Necrophaga Lamellicornes, Sternoxi, Malacodermata, Rhyncophora,* and *Longicornes,* &c.

As might be expected, their modes of attack on trees are as varied as their organisation, and their study is one of great interest to naturalists, besides being of the greatest importance to the owners of plantations and forests. Some species mine in the bark, others between the bark and the solid wood, others in roots and branches, and there are few, if any, of our native trees that are not liable to attacks from one or more species.

The amount of damage done to a forest when a few species get fairly established in it is very great, and too often the woodpeckers, which would assist in checking their ravages, are destroyed, because they bore holes in the trees to get at the insects.

At the present time the fine spruces in Dunham Park are being rapidly destroyed by one of the large weevils *Hylobius abietis*, and many ash trees by a small species mining in the bark, *Hylesinus fraxini*. In the valley of the Spean in Perthshire the alder trees are being destroyed by the larvæ of *Stenocărus bifarciatum*. They begin the work, which is joined in by some smaller species, to such effect that you may see there trees, 30 inches in diameter, through which you can thrust a walking stick.

A short time ago I accompanied a friend to his fishing cottage, in the north of Lancashire. It had been closed up for a little time, and the chairs and tables had been attacked by *Anobium striatum*, and although in appearance quite perfect, when touched almost crumbled to dust.

I bring for exhibition a piece of bark mined by two species of beetle, of great interest to entomologists, *Hylesinus vittatus* and *Nemosoma elongăta*. The latter species was taken in 1833 by Mr. Ingall, at Sydenham, and since then it has not been met with, except about three specimens, until the spring of this year, when my friend Dr. Power found it in Warwickshire and investigated its habits, publishing an account in the *Entomologist*. From his observations he ascertained that this species feeds on the larvæ of *Hylesinus vittatus*, carrying its galleries across so as to intercept and devour them. Having myself once found the *Hylesinus* in some old railings at Beeston, near Nottingham, when I was at the British Association meeting in August I again visited the place, and after careful search found some specimens of the rare *Nemosoma* in its mines across the tracks of *Hylesinus*. The portion of bark exhibited will show the mines, and the carded specimens will show the two species.

Stevens mentions Colwich near Nottingham, on the authority of Dr. Howitt, as a locality for *Nemosoma*; so no doubt by careful search it may be found in many other

places around Nottingham. I also exhibit some bits of oak branches from Dunham mined by *Scolytus intricatus*, and a few other species of wood-boring coleoptera found in this neighbourhood, with portions of wood attacked by them.

Scolytus destructor, the species which has been so destructive to elm trees near London and Paris, is not common here, probably in some measure because *Ulmus campestris* is not one of our common trees.

To the entomologist the investigation of the specific differences, the habits and instincts, and the peculiar conformation of these creatures to adapt them to their mode of life, are sources of great pleasure, but he is at the same time more impressed with the enormous amount of damage they inflict than an ordinary observer, and also with the want of knowledge of those who are interested in the preservation of our woods and forests. It may happen that the removal of a tree, or even a branch, when attacked by a particular species, may save a forest, but it must be done at the proper time, so as to destroy the insects with it before they can escape to propagate their species, and it certainly ought to be part of a qualification for a forester or park keeper that he should know the life history of these wood-boring beetles and the plans that have been adopted for their destruction.

Ordinary Meeting, December 26th, 1866.

EDWARD SCHUNCK, Ph.D., F.R.S., President, in the Chair.

Mr. PETER HART exhibited a piece of lead pipe from the Ardwick Bridge Chemical Works, coated internally with oxide of manganese. The pipe was one inch in diameter, and the thickness of the coating of oxide of manganese was about a quarter of an inch. The oxide has not been minutely examined, but it consists no doubt largely if not wholly of the hydrated peroxide, as it evolves chlorine in quantity when gently heated with hydrochloric acid. The water used in the works is collected in tunnels cut in the red sandstone, and there is no doubt that the source of the manganese is the refuse chloride from the chlorine stills situated almost immediately over one of the tunnels. A portion of this chloride, having filtered through the sandstone, has come into contact with the bicarbonate of lime in the water, chloride of calcium and carbonate of manganese have resulted, which latter body, by after contact with oxygen, has become wholly or partially converted into peroxide.

"On some recent observations on the Specific Gravity of Sea Water," by THOMAS HEELIS, F.R.A.S.

The author had in the course of several long voyages accumulated a number of observations on this subject, all of which were made at sea with the hydrometer. On coming to examine and reduce his results, he had referred to a paper by the late Professor Forchhammer of Copenhagen, published in the Philosophical Transactions. The

results arrived at by the Professor differed so much from those obtained by the author that a careful examination of the Professor's paper became necessary before any progress could be made in the reduction of the author's observations, as, if the results of the Professor were correct, the observations must involve such errors that their reduction would be useless. The examination was therefore undertaken, and was extended to the results of the American observations, which are shown in a curve given among the plates to Maury's Physical Geography of the Sea (9th edition). The American observations were found to accord with those of the author and to differ from the results obtained by the Professor to an extent in some cases of one per cent of the total salinity. The apparent cause of the discrepancy seemed to be the length of time during which some of the Professor's specimens were kept before being subjected to analysis, the paper in the Philosophical Transactions showing that in one case at least this interval amounted to some twenty years. A further examination of the paper showed, as the author conceived, great errors in arrangement, and especially in the determination of the areas, all the water taken in which, was assumed to be from the same region, and all the results of which specimens were included in a common average. There was a strange deficiency in the observations for certain regions, but the boundary lines of the areas were so arranged that the Gulf Stream was included in three regions, the L'Agulhas current in two, and the Equatorial current in two at least; so that if the results had not been drawn from too few specimens, the effect of the high specific gravity of these currents would have been entirely masked by the observations of other water not the subject of current, but included in the averages as belonging to the same regions. As it was, the same result, viz. the disappearance of the effect on the average of each region of the high specific gravity of the

waters of these currents was found to occur because of the small number of observations in the currents, there being only one, for instance, of the L'Agulhas current, and this one being masked in the average of the Indian Ocean region, the specific gravity of part of which, as is well known, is reduced by the fresh water of the large Asiatic rivers.

PHOTOGRAPHICAL SECTION.

December 13th, 1866.

J. Baxendell, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. Coote exhibited a frame for printing transparencies on glass, of which the following is a description :—

As frames for printing transparencies for the lantern and stereoscope have occupied some little attention of late, in the *British Journal of Photography*, I thought it might be useful to submit to the inspection of the meeting to-night one that I had made last winter for printing stereo-transparencies from negatives, on plates 7¼in. by 4½in., the size I always use on account of being able to take single pictures in the same camera. The frame is the same in principle as the usual frame for printing transparencies by superposition, and without having to cut either negative or print by simply moving the prepared plate and negative to the right and left in opposite directions alternately; but it is a little longer, and 4½in. broad inside. The opening for exposure is in the centre, but is made longer than is required for a stereo print. Over this opening I place a mat made of thin brass, the desired size and shape of the

picture required. This mat is sunk into the bed of the frame until it is quite level with it. A negative is then put in, and by moving the mat up or down in its recessed bed, you can get more sky or foreground at will, and the movement right and left gives you any other portion of the picture you may desire. Having settled that point, I then place on the negative a *straight* strip of soft cardboard, wide enough to reach, within about an eighth of an inch, the bottom of the opening in the mat; and then place on the prepared plate, which is kept in a horizontal position by resting on the strip of cardboard; and it also enables you to get the two sides of the picture exactly on a level with each other after reversing, to close the frame. A loose back, with a brass spring on, is put in, and a small lever bar brought down over the spring and held in its position by a small catch. It is necessary to have several strips of cardboard, varying in width, and it will be obvious to any one that they must not exceed in thickness the glass of the prepared plates, otherwise the pressure of the back is kept off the glass and the plates are not in close contact.

By use of this frame I can easily take out for the lantern portions of pictures taken the full size of the plate, say 7¼in. by 4½in., whether the subjects be horizontal or upright.

Mr. Dancer then exhibited, by means of the oxyhydrogen lantern, a very beautiful series of views, by Mr. Buxton, representing scenes in Scotland, Egypt, India, &c.; and also views in Derbyshire, Cheshire, and other places, by Mr. Coote and Mr. Brothers.

Ordinary Meeting, January 8th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. BINNEY, F.R.S., F.G.S., exhibited two remarkable fossils discovered by Mr. Joseph Tindall, of Thomas Street, Huddersfield, in the lower coal measures near that town. One was an insect, and, according to Mr. Tindall, belonged to Dr. Dawson's genus *Xylobius* and probably to his species *Sigillariæ*. It was found in an old deep mine at Cooper Bridge, and is the first instance of a specimen of that genus having been met with in England. The other bore some resemblance to the pupa state of a coleopterous insect, not much unlike the pupa of a nut weevil or some such insect. It was found in the Cinderfield Dyke Pit at Bradley, near Huddersfield. These specimens give us evidence of the former existence of insect life during the corboniferous epoch which a few years since we should scarcely have expected; but after the discovery of a fossil spider in the German coal measures scarcely to be distinguished from a recent genus, we must expect great additions to be made to the carboniferous *fauna*, as doubtless the rich and luxuriant vegetation of that remote period would afford food and shelter for numerous insects.

I. "On the amount of Carbonic Acid contained in Sea Air," by T. E. THORPE (Dalton Scholar in the Laboratory of Owens College), communicated by Professor H. E. Roscoe, F.R.S., &c.

With the exception of the results of some determinations already communicated to the Society by the author, the

only series of determinations hitherto published of the amount of carbonic acid contained in the air above the ocean are those of Lewy. At the request of the French Academy, Lewy, in 1848, collected the air of the Atlantic at different times during a voyage from Havre to Santa Marta, and determined the proportion of its three principal constituents in the eudiometric apparatus of Regnault and Reiset. The mean composition of the air of the Atlantic from Lewy's analyses is in 10,000 volumes,

Carbonic Acid.....	4·630
Oxygen	2102·750
Nitrogen.....	7892·620
	10000·000

These numbers differ but slightly from those usually given as representing the average composition of the atmosphere of the land, but on examining the experimental details of Lewy's analyses it appears that the air of the day was found to be considerably richer in carbonic acid and oxygen than the air of the night. The following are the means of each series :—

	The Day (7 Expts).	The Night (4 Expts).
Corbonic Acid.....	5·299	3·459
Oxygen	2105·801	2097·412

This remarkable difference appeared to become greater as the middle of the ocean was approached, where the air of the sea was less liable to be mixed with that of the land. These singular diurnal variations in the composition of the atmosphere of the sea are ascribed by Lewy to the evolution, and consequent admixture with the superincumbent air, during the day time, of dissolved gases from the heated surface-layers of the sea, such gases being considerably richer, as is well known, in oxygen and carbonic acid than ordinary air. During the night, on the other hand, the disengagement of gas is supposed to be arrested. Morren, and subsequently

Lewy, have shown that the changes in the relative amounts of the several gases held in solution in sea-water depend:— (1) upon the variations in intensity of direct and diffused solar light, producing a corresponding effect upon the vitality of sea plants and animals; and (2) upon the alterations in temperature affecting the relative amounts of the dissolved gases in accordance with the known laws of gaseous absorption. Consequently if such cases can at all influence the composition of the atmosphere of the sea it is reasonable to expect that the variations would be most perceptible in the air above the tropical oceans, in whose tepid waters infusoria exist in enormous quantities, and where the intensity of total sunlight is very great and its changes exceedingly rapid.

It would then appear from Lewy's experiments that sea-air is sensibly richer in carbonic acid than the air of the land, and that, contrary to the *a priori* statements of Vogel and Krüger sea-water does not abstract the carbonic acid from the air, but even causes a sensible increase in its comparative amount. Considering the difficulty generally experienced in accurately noting in the eudiometer contractions so minute as the absorption of the carbonic acid from a small volume of atmospheric air, it appeared desirable to test the validity of the above conclusions by a series of experiments made by one of the more convenient and accurate methods which we now possess for the estimation of atmospheric carbonic acid.

In a paper which had the honour of being read before the Society, the author communicated the results of a series of determinations of the amount of carbonic acid contained in the air over the Irish Sea. All these determinations were made by Pettenkofer's method, according to which the amount of atmospheric carbonic acid can be estimated with far greater accuracy than by any eudiometric method hitherto described. The details of the method of analysis were fully described in the paper above referred to. The

experiments on the air of the Irish Sea were, by the kind permission of the Honorable Board of Trinity House, made during the month of August 1865, on board the "Bahama Bank" light vessel, situated in Lat. 54° 21′ N, and Long. 4° 11′ W, seven miles W.N.W. of Ramsey, Isle of Man, and consequently nearly equidistant from the shores of England, Scotland, and Ireland. The times of observation were 4 a.m. and 4 p.m., the hours of nearly minimum and maximum temperature, when it was assumed that any differences in the air of day and night similar to those observed by Lewy over the Atlantic Ocean would be most perceptible. It appears from the 26 experiments embodied in the Table of Results accompanying the paper, that the mean quantity of carbonic acid in the air over the Irish Sea was in the month of August 1865, 3·08 in 10,000 volumes of air—a very marked difference from the mean amount on the land, viz., 4·0 vols. in 10,000, as deduced from the most extensive observations on land-air. It was therefore concluded that the Sea in our latitudes does not act in increasing the amount of carbonic acid in the air above the ocean, as found by Lewy over the Atlantic near the equator. And it also appeared from those experiments that the differences observed in the air of night and day by Lewy over the Atlantic are not perceptible in the air above the Irish Sea.

The kindness of Messrs. Alfred Booth and Co., of Liverpool, has enabled the author to test the validity of Lewy's conclusions respecting the composition of the atmosphere above the tropical oceans by a series of determinations of the amount of carbonic acid contained in the air of the Atlantic, made by Pettenkofer's method, during a voyage to and from the Brazils. The bottles employed in collecting the air were however of increased capacity, since by thus experimenting on a larger volume of air any variations in the amount of atmospheric carbonic acid would, of course, be rendered more appreciable. From 51 experiments made

during the months of February, March, and July 1866, it appears that the mean quantity of carbonic acid contained in the air of the Atlantic Ocean is 2·953 vols. in 10,000 of air: in that of the Irish Sea it was, from 26 experiments, 3·082: the mean of the 77 experiments being 3·00.

This result compared with the following numbers giving the amount of carbonic acid contained in land air, shows that, contrary to the statement of Lewy, the air over the ocean contains a much smaller proportion of carbonic acid than that of the land.

The most extensive observations on land air have given as means :—

Observer.	Locality	No. of Expts.	Vols. in 10,000 in air.
Th. de Saussure...	Chambeisy	104	4·15
Boussingault	Paris	142	3·97
Verver	Gröningen	90	4·20
Roscoe	London and Manchester.	161	3·95
Angus Smith	Ditto ditto	200	4·03
General mean of observations on land air			4·04

	Independent Expts.	
General mean, observations on sea air, Lewy...	11	4·63
Ditto ditto ditto Thorpe	44	3·00

The quantity of carbonic acid contained in land air is subject to continual alteration from the variable circumstances of locality, temperature, fog, rain, &c.; it may thus vary from 2·5 to 8 volumes in 10,000 of air. It would appear, however, from the above experiments that the amount of carbonic acid in sea air experiences fewer and far less extensive variations: it is sensibly the same in different latitudes and is constant in the same locality throughout the year. The mean quantity of carbonic acid contained in sea air between the parallels 15° and 30° N. latitude was in the months of February and March 1866, 2·96 in 10,000 volumes of air; in July of the same year it

was 3·00 volumes. From Saussure and Boussingault's observations a decided difference may be traced between the amounts of carbonic acid in the air of day and night on the land, the air of the night containing more carbonic acid than that of the day in the proportion of 100 to 92; but, on comparing the experiments made at night with those of the day, it appears from the author's observations that no such differences are to be discerned in sea air. The means of the two series are—

	For the Day. (24 observations)	For the Night. (20 observations)
Carbonic acid in 10,000 vols. of air...	3·011	2·993

The non-accordance of these results with those obtained by Lewy is undoubtedly mainly due to the difference in the methods of analysis employed. Lewy's determinations were made in Regnault's eudiometer in which, as is well known, the volume of gas is maintained constant throughout the analysis, the relative changes being estimated by the variations in the pressure of that constant volume over or under that of the atmosphere, measured in millimetres of mercury; and thus, as Dr. Frankland as already pointed out, a variation in volume, which in the older method of Bunsen would appear considerable, is in that of Renault only represented by a small numerical expression, and consequently a very slight error of observation (say to the extent of one-twentieth of a millimetre) would exercise a very considerable influence on the result. It deserves also to be noted that Lewy's analyses were not made until eighteen or twenty months after the air had been collected, and although Lewy satisfied himself by experiment that air contained in glass tubes for such a length of time experiences no alteration in composition, yet Regnault has subsequently shown that the exact determination of the amount of atmospheric carbonic acid cannot be made in air thus preserved, since the glass absorbs a portion of this gas.

The main conclusions to be drawn from this paper may be briefly stated as follows :—

1. That the sea does not act in increasing the amount of atmospheric carbonic acid.
2. But that, on the contrary, the air over the sea contains a much smaller proportion of carbonic acid than the air of the land, although the influence of the sea in abstracting this gas from the atmosphere is not so great as the observations of Vogel and Krüger would indicate.
3. That the mean quantity of carbonic acid contained in the normal atmosphere of the ocean is 3·00 in 10,000 volumes of air.
4. That the proportion is constant, or nearly so, in different latitudes.
5. That this proportion is not sensibly influenced by the different seasons of the year.
6. That this proportion does not experience any perceptible diurnal variation.

II. "On the Amount of Carbonic Acid contained in the Atmosphere of Tropical Brazils during the rainy season," by T. E. Thorpe.

The determinations contained in this communication of the amount of carbonic acid in the land air of the tropics were made at Pará, the principal port of entrance of the river Amazon, situate about eighty miles from the sea, on the river Grâm-Pará, in lat. 1° 27′ S., and long. 48° 28′ W. This town is built on the verge of a vast primeval forest, extending to the sea coast, and over which the trade winds of the Atlantic regularly blow during the greater part of the year. The determinations were made by Pettenkofer's method, in the manner previously described. Hydrochloric acid was, however, substituted for oxalic acid in the titration of the baryta-water, since solutions of the latter acid decompose

rapidly in the tropics. From 31 experiments it appears that the mean quantity of carbonic acid contained in the air of tropical Brazils during the months of April and May 1866 was 3·28 in 10,000 volumes of air—a decided difference from the mean proportion (4·0 vols. in 10·000 of air) contained in the atmosphere of Europe, and probably due in great measure to the joint action of tropical rain and tropical vegetation in withdrawing this gas from the air. The influence of rain on the amount of atmospheric carbonic acid is well known from the experiments of Saussure and Boussingault. The annual rainfall at Pará is very heavy, amounting, according to observations kindly furnished to the author by Drs. Bruno-Cabral and José Abreu to nearly 3 metres (118 inches), of which about one-third falls during the months of March, April, and May. These determinations are consequently interesting as showing the proportion of carbonic acid in the atmosphere of the tropics in the middle of the rainy season of the year.

In conclusion, the author begs to tender his thanks to Professor Roscoe for the valuable advice and assistance he has given him during the above investigations.

PHYSICAL AND MATHEMATICAL SECTION.

November 8th, 1866.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

Mr. Baxendell, F.R.A.S., read a paper entitled "Observations of the new Variable Star T Coronæ." (This paper was afterwards read at the Ordinary Meeting of the Society held November 27th, 1866. See page 35.)

Mr. Brothers, F.R.A.S., communicated an Appendix to his "Catalogue of Binary Stars" presented to the Section at its last meeting.

December 6th, 1866.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

Mr. Heelis, F.R.A.S., exhibited some drawings of a Hydraulic Clock which he had made for the purpose of driving his Equatorial, and described the nature of its action and adjustment, the principal feature being that of using the same water over and over again.

Mr. Brothers, F.R.A.S., communicated the following observation of the occultation of Aldebaran, which took place on the 22nd ult.:

Disappearance, $1^h\ 53^m\ 32{\cdot}5^s$ local sidereal time. The star appeared projected on the moon's limb for at least half a second, and then disappeared instantaneously.

Position of the Observatory: Lat. 53° 27′ 9″ N; Long. $0^h\ 8^m\ 54{\cdot}5^s$.

Mr. Heelis, F.R.A.S., read a paper "On some recent results of Observations of the Specific Gravity of Sea Water." (This paper was afterwards read at the Ordinary Meeting of the Society held on December 26th. See page 55.)

Mr. Vernon, F.R.A.S., exhibited a diagram showing the excessive amount of barometric and thermometric oscillation which had occurred during the month of November, 1866.

January 3rd, 1867.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

"On the Mean Weekly Temperature at Old Trafford, Manchester, for the seventeen years 1850—1866," by G. V. Vernon, F.R.A.S., F.M.S.

As I am not aware that there have been any carefully deduced values of the mean temperature of this neighbourhood, perhaps the data accompanying this paper may serve until such time as a more extended series can be obtained. I may state that the thermometers used have all been standard ones compared at Greenwich, and all the observations have been reduced to that standard. The thermometers are placed upon a stand 4 feet from the ground, and carefully protected from radiation and other disturbing influences.

The unavoidable omissions in my register I have been enabled to supply by the kindness of my friend Mr. John Curtis, F.M.S., whose thermometers are placed similarly to my own, and within a very short distance from my station. The mean values have generally been determined from the readings of the maximum and minimum thermometers in the shade combined with readings of a standard thermometer read once a day; these observations being all made at 8 a.m.; in reducing them Mr. Glaisher's corrections for diurnal range have invariably been applied. Whilst upon this subject I

would like to suggest the great desirability of having these corrections deduced from a much larger series of observations, for although on the whole I find them to agree pretty closely, yet at times great differences exist, especially in comparing the mean temperature deduced from maximum and minimum readings with those of a standard thermometer read at certain fixed hours.

The coldest weeks during the 17 years average appear to be those ending 7th and 14th January, and the warmest that ending July 22nd.

The week ending December 29th appears to be the one in which the greatest variation of mean weekly temperature is likely to occur, and the one ending August 19th that in which the least variation occurs.

Taking the mean differences for each month, and we obtain the following figures :—

	Mean Differ.		Mean Differ.
January.....	16·40°	July	13·22°
February	18·62	August	8·20
March	13·40	September.....	10·26
April	12·05	October	11·34
May	14·37	November.....	13·62
June	12·42	December	20·22

From this table we see that the greatest amount of variation occurs in December, and the least amount in August.

October does not appear to exhibit any abnormal variation, although from the amount of barometrical oscillation doing, so it might have been expected it would do.

I should like to see a much longer period observed for determining the values of the mean weekly temperature here, but hope until such is the case the values I have given may be deemed worthy of the confidence so short a period can deserve, and I can safely say, that as the great mass of the observations were made by myself, every care has been taken in order that they might be made correctly.

Mean Weekly Temperature at Old Trafford, Manchester.

106 Feet above Sea Level.

Week Ending	Mean Temp. 17 Years, 1850–1866.	Difference between Warmest and Coldest Week.	Week Ending	Mean Temp. 17 Years, 1850—1866.	Difference between Warmest and Coldest Week.
January 7 ...	37·5°	21·7°	July 8	58·3°	10·7°
,, 14 ...	37·5	15·0	,, 15	60·6	16·9
,, 21 ...	38·3	13·9	,, 22	61·0	12·1
,, 28 ...	38·7	15·0	,, 29	59·8	13·2
February 4 ...	38·0	14·2	August 5	58·6	7·7
,, 11 ...	39·0	14·3	,, 12	59·6	10·6
,, 18 ...	38·5	22·8	,, 19	57·8	7·2
,, 25 ...	37·7	23·2	,, 26	58·0	9·1
March 4	39·8	13·8	September 2...	57·8	6·4
,, 11	39·5	14·1	,, 9...	57·5	7·6
,, 18	40·8	10·8	,, 16...	56·1	13·4
,, 25	41·0	12·2	,, 23...	54·5	13·8
April 1	42·4	16·3	,, 30...	53·3	10·1
,, 8	45·4	14·1	October 6 ...	53·2	7·5
,, 15	46·5	8·2	,, 13 ...	49·8	11·2
,, 22	48·0	15·1	,, 20 ...	49·0	12·7
,, 29	48·0	10·8	,, 27 ...	47·1	16·6
May 6	47·3	12·8	November 3...	44·7	8·7
,, 13	49·3	12·9	,, 10...	43·1	14·0
,, 20	52·8	18·4	,, 17...	40·4	12·6
,, 27	54·4	13·4	,, 24...	40·6	16·8
June 3.....	54·7	12·6	December 1...	39·5	16·0
,, 10.....	56·8	11·1	,, 8...	40·1	14·3
,, 17.....	57·2	14·1	,, 15...	40·7	20·9
,, 24.....	58·1	10·9	,, 22...	39·0	18·6
July 1	60·0	13·4	,, 29...	39·0	27·1

"On the Rainfall for 1866, at Old Trafford, Manchester," by G. V. Vernon, F.R.A.S., F.M.S.

Rain Gauge 3 feet above the Ground, and 106 feet above Sea Level.

Quarterly Periods.		1866.	Fall in Inches.	Average of 73 Years.	Differences.	No. of Days' Rainfall in 1866.	Quarterly Periods.		Quarterly Periods for 73 Years.
1865.	1866.						1866.	1865.	
Days	Days		In.	In.	In.		In.	In.	In.
		Jan. ...	3·252	2·480	+0·772	22			
48	59	Feb....	2·983	2·387	+0·596	20	8·403	7·143	7·166
		March.	2·168	2·299	−0·131	17			
		April...	0·299	2·001	−1·702	7			
36	32	May ...	1·540	2·356	−0·816	9	5·814	5·226	7·258
		June...	3·975	2·901	+1·074	16			
		July ...	4·309	3·571	+0·738	13			
36	67	Aug....	5·119	3·616	+1·503	26	16·556	7·502	10·441
		Sept ...	7·128	3·254	+3·874	28			
		Oct.	2·521	3·816	−0·295	13			
44	56	Nov....	5·721	3·494	+2·227	20	12·396	8·518	10·548
		Dec. ..	4·154	3·238	+0·016	23			
164	214		43·169	35·413	+7·756	214	43·169	28·389	35·413

The rainfall for 1866 has been 7·756 inches above the average of the last 73 years, and 14·780 inches above the fall for 1865. The fall occurred upon 214 days in 1866, as compared with 164 days in 1865.

The principal excess occurred in the three months of July, August, and September, the fall in the latter month being the greatest fall for September during the entire period. In the months of April, May, and June the total fall was considerably below the average, June, however, taken alone, having an excess, but April had an unusually small fall.

With the exception of October, the rainfall was greatly in excess from June to December, and this would tend still further to bear out the supposition that October is subject to some peculiar abnormal disturbances, as pointed out by Mr. Baxendell (*Memoirs*, 3rd Series, Vol. I., page 263), and also by myself (*Proceedings*, Vol. V., page 87), in former communications.

During the later months of the year there were excessive barometric and thermometric disturbances, as shown by the amounts of oscillations.

The small amount of rainfall in October this year is very remarkable, as it is on an average the wettest month of the entire year, as will be seen on comparing the annexed figures.

The daily falls exceeding half an inch were as follows:—

	Inches.		Inches.
January 21	0·568	September 2	0·500
May 11	0·760	„ 8	1·150
June 18	1·006	„ 11	0·650
July 3	0·610	„ 20	0·550
July 28	1·985	„ 28	0·770
August 1	0·900	October 24	0·523
„ 16	0·670	November 15	1·240
„ 29	1·084	„ 16	0·802
September 1	0·750	December 3	0·580

The great fall of November 15 and 16 was accompanied by the great flood of the Irwell, the Irk, and the Medlock. A fall of half an inch in this neighbourhood may be considered excessive, and even the above table only gives 18 days on which the rainfall reached this amount, during what may be regarded as a very wet year. Examination of the figures given by Mr. Curtis, F.M.S., in his paper printed in Vol. XV. of the 2nd Series of *Memoirs* of our Society, gives the following years in which the rainfall exceeded 40 inches since 1794:—

	Inches.		Inches.
1823	44·767	1836	45·551
1824	42·941	1841	41·190
1828	45·267	1845	41·415
1830	40·861	1847	45·230
1833	41·677	1852	45·730

In 1860, which was generally considered a very wet year the rainfall was only 36·530 inches, but fell upon 235 days, or 21 more than this year (1866), the principal difference being that in 1860 rain was continually falling, though often not much in amount, whereas this year rain has fallen on a large number of days, and often very heavily.

"Results of Observations of Variable Stars made at the Mannheim Observatory during the year 1866," by Dr. E. Schönfeld, Director of the Observatory. Communicated by Joseph Baxendell, F.R.A.S.

The number preceding the name of each star in the following list is the ordinal number of the star in the Author's "Catalog von veränderlichen Sternen mit Einschluss der neuen Sterne. Mit Noten."

6. *S Cassiopeæ.*—The last maximum occurred 1866, August 23. The period will be about 573 days.
8. *R Piscium.*—The approaching maximum appears likely to occur considerably sooner than the elements require.

15. *R Persei.*—The maxima of the present year occurred on April 27 and November 20; the first, however, was observed in the evening twilight, and the time may in consequence be slightly in error. It appears, therefore, that the period is about 205·5 days, and that one period must be added to the number hitherto assumed to have happened since the date of Bessel's observation. This correction, however, does not accord with an observation made at Bonn 1853, November 30, where the star was omitted in one of the zones observed by me, although it would then be near a maximum. But it was on that evening bright moonlight.

24. *R Aurigæ.*—This star has again had another double period. A maximum took place March 4, mag. 8·8; a minimum, June 7, mag. 9·4; and then a principal maximum on August 26, mag. 7·0. The interval between my two observed principal maxima is 463 days.

30. *R Canis minoris.*—In its last minimum, 1866, March 10, this star was of the 10th magnitude. The observation of Oudemans, according to which it was then invisible, appears to rest upon an error.

35. *U Geminorum.*—A very short maximum of this star occurred in April, 1866. On the 15th it was of its ordinary minimum brightness, or at most, but a very little brighter than usual. April 16 and 17 the weather at Mannheim was cloudy.

April 18, at	8h	18m	M. T. Mannheim, U was=	9·5	mag.
	11	3	„ „	9·4	„
19,	8	29	„ „	9·7	„
	10	36	„ „	9·7	„
20,	10	41	„ „	10·2	„
21,	8	58	„ „	11·1	„
23,	8	36	„ „	13·0	„

These observations give the epoch of maximum = 1866, April 18·5.

39. *S Cancri.*—My Ephemeris in No. 1593 of the "Astronomische Nachrichten" is proved to be very nearly correct by a well-observed minimum of this star 1866, November 29, $13^h\ 3^m$ M. T. Mannheim = $12^h\ 42{\cdot}7^m$ M. T. Paris, at the earth's mean distance.

55. *V Virginis.*—A maximum 1866, April 13, occurred 23 days earlier than my elements indicated, and a comparison with Mr. Pogson's epoch for 1860 gives the period = 249·4 days; but Goldschmidt's earlier observations appear to show that the period must be increased to 251 days.

59. *S Bootis.*—A careful examination of my observations gave a maximum 1865 July 11·5; and a good determination of the following maximum gave the epoch, 1866, April 17. The corrected elements will therefore be—1865, July $14 + 275^d$ E.

63. *S Libræ.*—Schmidt has communicated to me his observations of the past summer, which have enabled me again to improve this star's elements. At the same time I satisfied myself on the 9th of June, 1866, that $2^h\ 8^m$ after the epochs given in my Ephemeris a second minimum occurs. I believe that the value multiplied by E in my new Elements

Minimum 1866, April 12 $15^h\ 25^m$ M. T. Paris
$+\quad 6\ 23\ \ 33\ \ 38{\cdot}39$ E

is three times the value of the real period, which would therefore be = $2^d\ 7^h\ 51^m\ 12{\cdot}80^s$, with a probable error = $\pm 2{\cdot}057^s$. This is a very interesting star.

64. *S Serpentis.*—The maximum of this year occurred on February 26, or exactly one year after the preceding maximum. It appears, therefore, that the period is yet longer than that given by Argelander; also, that

this inequality of the period begins to show itself as a periodical one.

71. *R Scorpii.*—Maximum, 1866, May 5.—The observations commenced only on the 24th April, but are not unsatisfactory. However, it is possible that this maximum (11 mag.) was by no means the proper one, but only a secondary one during the waning of the star, and that the proper maximum occurred much earlier.

72. *S Scorpii.*—Maximum observed 1866, June 17, tolerably good. Interval from previous year's maximum = 355·5 days.

77. *S Ophiuchi.*—The elements are better than I expected, for they gave a maximum on the 20th June, 1866, whilst the epoch from my observations was June 24·5.

78. *S Herculis.*—Last maximum 1866, October 11, apparently good, but 11 days earlier than the Elements give.

89.—*T Sagittarii.*—The epoch of maximum for this year as concluded from the course of the light-curve is September 14 but I had no observations of this star from August 23 to September 28. I believe, however, that the result given cannot be many days in error. The period appears, therefore, to be rather less than one year.

90.—*R Sagittarii.*—Observed maximum 1866, July 13. The period is therefore 269¾ days. Dr. Auwers had already informed me that Pogson's number, 467 days, was erroneous.

91. *S Sagittarii.*—Maximum 1866, October 4·5, very good. The period is to me still unknown.

101. *R Delphini* (*S Aquilæ*).—Observed maxima 1865, August 30·5; and 1866, May 27·5. Interval 270 days. Although both determinations are good, the period, if regular, cannot be under 279 days.

104. *S (R) Delphini.*—My observations of the last maximum give for epoch 1866, October 18·5, which is apparently very good, but it is by no means adapted to a period of 283 days.

105. *T (S) Delphini.*—Last maximum 1866, July 10·5, which is in fair accordance with the Elements.

113. *T Pegasi.*—Observed maximum 1866, October 31. It appears, therefore, that the period is considerably shorter than as given in the Elements.

115. *S Aquarii.*—A well-observed maximum 1866, October 27·5, confirms the correctness of the Elements.

117.—*R Pegasi.*—Observed maximum 1866, July 9. Argelander has communicated to me the following maxima derived from his earlier observations:

1848, November 7, doubtful
1850, December 7
1851, December 20
1853, January 6
1854, January 21

These are all very well represented by the Elements 1856, February $17 + 379{\cdot}5^{d}$ E; and it appears, consequently, that Oudemans' maximum 1855, January 26 or 27, was observed under favourable circumstances, notwithstanding the star's proximity to the horizon.

119. *R Cassiopeæ.*—The last maximum again confirms the probability of the conclusion that the period is now becoming shorter. According to my observations it occurred 1866, April 10 (the previous one 1865, February 21·5), and the star was brighter than I had ever before seen it. It was nearly equal to λ Andromedæ, and, in spite of its low position near the northern horizon, was very good to see with the naked eye.

PHOTOGRAPHICAL SECTION.

January 10th, 1867.

Dr. J. P. Joule, F.R.S., &c., Vice-President of the Section, in the Chair.

Mr. Coote exhibited a selection from the series of photographs by Braun, of Paris, consisting of copies of drawings by the old Masters, from the originals in the Louvre. Mr. Coote explained the method by which they were supposed to be printed, and from the appearance of some of the prints there could be no doubt that one of the processes suggested by Sir John Herschel about the year 1840, or some modification, had been employed; and it was evident that the method proposed by Mr. Gatty would also produce similar results. The prints are remarkable for their perfect resemblance to old drawings and sketches, and, as studies for artists and for use in schools of design, are quite equal to the originals. Of their permanence there cannot be the least doubt.

The following letters from Mr. J. B. Dancer, F.R.A.S., addressed to the Chairman, were then read by the Secretary:—

1.—I am too unwell to attend the meeting of the Section this evening, and have therefore put on paper a few remarks on "Braun's" copies of drawings of the old Masters.

Mr. Coote kindly showed me these pictures, and my first

impression was, that they had been produced by Mr. Swan's process, as it is quite possible to replace the carbon by coloured pigments, and imitate the colour of any chalk, crayon, or even black lead pencil, in the latter case using plumbago. The act of rolling a picture of this kind would give it a lustre like a pencil drawing. There are specimens of this appearance in the collection.

Having had occasion to write to Mr. Swan, I requested him to give me such information as he possessed as to how these copies were produced. The following is an extract from his reply:—

"To J. B. Dancer.

"Many thanks for your kind enquiry. Mr. Braun, of Dornach, is printing a number of copies of pictures by my process, in particular a series of Holbein's works; and if this series form a part of the collection to be exhibited at your society's meeting, they are no doubt by my process. Mr. Braun is however also using another process (Rousseau's) for the production of the Louvre pictures, and I think it most probable that all the red pictures, except the Holbeins, are by this process. I do not know precisely what M. Rousseau's process is, but I have heard that it is a modification of one of the processes of Sir John Herschel.

"You are quite right in thinking my process suitable for the imitation of old red chalk drawings; the shades of colour can be imitated exactly.

"Mr. Braun is now carrying out my process very energetically, and has erected a steam engine to drive the tissue making machinery. "Joseph W. Swan."

I think, Mr. Chairman, you will agree with me, when looking over these copies, that they mark a most important advancement in the art of Photography. In these productions we have identity in colour as well as in form.

2.—At a meeting of the Photographical Section, October 4th, 1866, it may be remembered that I exhibited some specimens of photographs in various colours, on paper and calico, a process invented by Mr. John Mercer, F.R.S. I did not recollect sufficient of what Mr. Mercer had told me of the process to give you any definite information as to the production of the colours. A few days since I stumbled on his description; if it is thought of sufficient interest, please to communicate it to the members.

Mr. Mercer read a paper on the subject at the meeting of the British Association at Leeds, in the year 1858. He exhibited coloured photographs on paper and calico, and gave the following directions for producing them.

DIRECTIONS.

34oz. of sulphate of iron are converted into peroxalate; this is diluted to 2 gallons, and will impregnate 200 square yards of paper. The paper being floated on the solution until fully wet in the usual way, it is then exposed, and afterwards steeped in some solution which only acts on that part where the iron has been reduced from the per- to the protoxide. Red prussiate of potash and sulphuric acid act well, making the image blue and the ground white.

Sulphocyanide of potassium and a salt of copper form another bath; the protoxide of the picture deoxodises the copper, and the sulphocyanide of the suboxide of copper is fixed in the cambric or paper. This may be converted into the red prussiate of copper.

A vast number of colours may be obtained by replacing the iron or copper by other metals, such as lead, zinc, tin, mercury, silver, gold, or manganese.

With these bases may be used various dyes, as madder, cochineal, murexide, logwood, galls or quercitron bark, besides the iodides, chromates, prussiates, or oxides of the metals themselves and mixtures of these.

He also showed how the peroxalate paper might be used as a fair actinometer, by placing a slip between the leaves of a book and pulling it out by steps every stated number of seconds. It is then easily converted into a graduated scale.

Ordinary Meeting, January 22nd, 1867.

Dr. J. P. Joule, F.R.S., &c., Vice-President, in the Chair.

Mr. Sidebotham called attention to the letters which had appeared in the "Times" newspaper during the last few days, with reference to chilled cast iron shot and shell, intended for penetrating armour plated ships, and as to whom the credit of first suggesting it rightly belonged. He said that no doubt whatever existed that at the Meeting of the British Association, in October, 1862, their member, Mr. Nasmyth, C.E., did most strenuously urge a speedy trial of chilled cast iron elongated shot and shell, as being not only much cheaper than steel, but better in every respect for the purpose of penetrating iron-clad ships; he also gave his reasons for such an opinion, and held his own during a long discussion against all comers.

Mr. Sidebotham then read letters to Mr. Nasmyth from P. Le Neve Foster, Esq., who acted as secretary to section G at the Cambridge meeting of the British Association, and T. Aston, Esq., Q.C., who took part in the discussion. The latter concludes his letter by saying:—

"There can be no doubt whatever, that any one who may claim to have been before you in teaching the public the use of chilled cast iron for projectiles intended to penetrate armour plates, must give proof of having done so prior to your vigorous advocacy of that material at the Cambridge meeting of 1862."

Mr. Sidebotham then read reports of the discussion from the "Times" and the "Athenæum;" both these are short, as such reports usually are, and do but represent a very small portion of what was said. The report in the "Times" of October 7th, 1862, is as follows:—

"Mr. Nasmyth inquired whether chilled cast iron shot had been tried, the process of chilling cast iron was a very inexpensive one; and if chilling flat ended shot had not been tried, it was very desirable it should be.

"The President (Mr. Fairbairn) said they had no practical experience in the matter; but he believed that the shot being chilled only to a certain extent they would find that the whole shot, having the same velocity when striking the object, would break, as if it had not been hardened at all.

"Mr. Nasmyth: That is a mere matter of opinion. (Hear, hear.) Have you tried any experiments on the subject? It would involve an expense of only some ten shillings. (A laugh.)

"The President: In order to gratify Mr. Nasmyth, I hope that before the next meeting of the British Association we shall have the matter proved experimentally. (Hear.)"

Mr. Sidebotham, in conclusion, said he thought from the above it would be seen that their member, Mr. Nasmyth, ought to have the credit of the suggestion of elongated chilled iron shot and shell, in place of steel, rather than Major Palliser, whose experiments and publication dated nine months later. He regretted the absence of Mr. Fairbairn from the meeting this evening, as he was the chairman of the meeting at Cambridge, and could have fully corroborated all that had been said.

"On Casting, Grinding, and Polishing Specula for Reflecting Telescopes," Part II., by JAMES NASMYTH, Esq., C.E., Corresponding Member of the Society.

In this part of his paper the author gives full details, illustrated by diagrams, of the methods which after many years' experience he has found to be best adapted for ensuring success in the fine-grinding and polishing of specula for reflecting telescopes.

Storm Warnings.

Since the meeting, Mr. Vernon, F.R.A.S., has forwarded to the Editor a letter from Dr. Buys Ballot, of Utrecht, in which the writer calls attention to the fact that the system of issuing storm warnings was first suggested and carried out by himself at Utrecht, and not by Admiral Fitzroy in London, as is generally supposed in this country. He says, "I laid down my principles before the Dutch Academy of Sciences in October, 1857; in the *Comptes Rendus de l'Académie des Sciences de Paris* in November, 1857. I made my first proposal to the Dutch Government on the 14th of November, 1859, and the system was commenced on the 1st of June, 1860, or nine months sooner than in England. I communicated a paper on the subject to the British Association at the Newcastle meeting, and I wonder that my system has not been adopted in England, the more so because I recommended it to Mr. Francis Galton in August last. It is not only a right of priority that I claim; but I think my system is much better, though it gives no particulars as to rain or snow, but only the direction of the wind for the next twenty-four hours almost without fail, and the force or strength of the wind with great probability. *It is a fact above all doubt* that the wind that comes is nearly at right angles to the line between the places of highest and lowest barometer readings. The wind has the place of lowest height at its left hand, and is stronger in proportion as the difference of barometer readings is greater. If Portsmouth and Valentia rise much above Nairn, then I think you will have a gale from the west, and you have to attend to the oscillation of that difference of barometer readings. The only question is how many hours does that difference take place before the wind comes. Now, I propose to introduce an instrument that in all our ports will simply show the azimuth in which the greatest difference of barometer readings prevails, and the amount of that difference.

The sailor acquainted with the meaning of the signs will have to look at that instrument at the same time as the barometer. It is a new instrument that may be regarded as a differential barometer showing the state of the atmosphere in the surrounding countries.

"For many years weather signals after my method have been exhibited every morning at nine o'clock at Utrecht; the strength and direction of the wind are observed every hour and compared with the prognostics so that I could say how many times the rule has held good, and how many times it has failed. Why does not the British Government send a distinguished meteorologist to Utrecht in order to see what we have done? I do not pretend to have found the whole truth, and acknowledge that the rule may admit of exceptions in other countries; but in Holland we have had more experience of the prognostics of gales and storms than all other countries put together, for here all is reduced to measures and time; in England and France all is vague and in general expressions."

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 7th, 1867.

A. G. LATHAM, Esq., President of the Section, in the Chair.

The PRESIDENT exhibited mounted specimens of Foraminifera from Dogs Bay, Roundstone, and from Berwick Bay, and he remarked on some differences in the character of the two deposits. He pointed out that in the Dogs Bay sand the prevailing forms are Truncatulina and Miliolina, while in the Berwick Bay dredging they are Dentalina and Biloculina. He also remarked on the great difference in the quality of the shell of Truncatulina from these two localities, those from Dogs Bay being delicate and hyaline, while those from Berwick Bay are opaque and very thick and strong.

The following paper, "On Polymorphina tubulosa," was read by Dr. ALCOCK.

In the course of examinations of the Dogs Bay sand, I have collected great numbers of detached branches of Polymorphina tubulosa, a form of foraminifer which is not likely ever to be found perfect in shore sand. I have, however, met with several fine specimens of it with only the tips of the branches broken away; but the most interesting examples are some which are more damaged, and show several structural features difficult, if not impossible, to be seen in perfect specimens. The main body of the shell of Polymorphina tubulosa has the form of Professor Williamson's P. communis, and appears to be identical with it, this form only, so far as I have seen, taking on the peculiar final

development characteristic of P. tubulosa. It consists in the mature state of the rounded shell of P. communis more or less concealed by several covered passages commencing at the mouth and taking a direction towards the base of the shell. These passages have their arched walls developed into tubular prolongations, extending in all directions, and soon dividing irregularly into small branches, which, in one or two instances in the specimens shown, will be found to anastomose : they are either closed at their tips, as a small glass tube might be closed in the flame of a blowpipe, or they expand into little cauliflower-like excrescences, which are also apparently closed. The shell composing the parts just described is very delicate and thin compared with that forming the rounded nucleus, and its outer surface is frosted with small glassy projections of an irregularly squared figure, like imperfectly formed crystals. It is evident that this is a hastily deposited shell-covering on the sarcode developed since the last regular chamber of the shell was formed, and which, instead of collecting itself into a definite shape to produce a chamber similar to the others, had been surprised, as it were, while fully expanded by the calcifying process, which consequently gives us a petrified representation of the ordinary appearance of this external sarcode, with its pseudopodia protruded, the probable suddenness of the process being illustrated by the cauliflower excrescences which terminate many of the branches, and which have resulted from the contraction of the extremely fine terminal filaments of sarcode. It would appear that this is the final act in the life of the Polymorphina, its enfeebled vital power being insufficient to gather together the sarcode for the formation of another regular chamber, and therefore, properly speaking, the shell is fully formed and perfect before this last addition is made to it. There is evidence, however, in the specimens I have now to show, that the animal must have lived for a considerable time in

a full-grown state before it thus terminated its existence, by producing a permanent likeness of its living self. These specimens have their arched coverings, with the branches proceeding from them, more or less broken away, so as to expose the floor beneath them, which consists of parts of the strong outer wall of the rounded nucleus, and which in all the cases examined presents the same peculiar appearance. It is riddled through with many large holes sometimes nearly circular but oftener oval or kidney-shaped, and so numerous as to open a very free communication between the external sarcode and that in the interior of the shell. It is not unusual to find Polymorphinas of a different type from these with a few small round holes in their outer walls, but they are scattered irregularly, are few in number, and have no evident relation either with one another or with any structural peculiarity of the animal; whereas in the present case they are invariably contained within the area of the floor of the covered passages, and are so numerous and encroach so much on each other that in some parts they leave only narrow isthmuses of the original shell-wall between them, and the larger holes have every appearance of having been formed by the union of several smaller ones. It is evident from a consideration of their character that they have been produced by the removal of shell-material previously deposited, and this gives them a physiological interest, for though it is natural to suppose that a creature which has the power of precipitating carbonate of lime on its surface would also have the power of removing portions of it by solution or absorption if required, the foraminifera are so structureless that we should hesitate to attribute to them this function without clear and positive proof.

In order to follow the successive changes in the latter part of the life of this Polymorphina, as they are illustrated in the specimens before you, the large rounded shells of P. communis should be first noticed, in which no opening is

perceptible excepting the mouth, showing that at this stage the numerous large holes which are afterwards formed have no existence. The great thickness of the outer walls compared with that of the internal parts of the shell shows that the animal must have existed for a considerable time in this condition, during which the surface has been strengthened by repeated deposits of calcareous matter from its coating of external sarcode, and the smoothness and evenness of this surface shows that the coating was at that time spread uniformly over the whole of it. But broken specimens of P. tubulosa show that a change in the disposition of the external sarcode has been afterwards made, for in these it is found to have collected itself into two or three irregular bands, always commencing by one end at the mouth and extending towards the base of the shell, an arrangement clearly mapped out by the remains of its ultimately formed shell-covering, fragments of which are seen still attached to the surface of the smooth rounded nucleus.

The next event in the life of this Polymorphina is the formation of those numerous openings through the thick shell-walls, the observation of which in the specimens before you has chiefly led me to introduce them to your notice. These show, by their definite position and the evidence they give of their progressive formation, that when the external sarcode has once taken the form of bands it remains permanently in that state, and that these bands hold a fixed position on the parts of the shell where they were first placed. Among the specimens shown are some which only differ from ordinary shells of P. communis in being remarkably smooth on the surface and in having numerous large holes arranged in several rows radiating from the mouth towards the base of the shell, exactly as in undoubted specimens of P. tubulosa, but they are without the slightest trace of the external arched coverings and

tubular branches. These might at first sight be set down as very much rolled and worn specimens of the ordinary P. tubulosa, but there is no evidence in the Dogs Bay sand of other kinds of foraminifera being worn to the extent which would be necessary to produce such a result, and the suggestion is uncalled for in this particular case since it is evident that at one part of the life of the animal its shell must have presented the appearance of these specimens, unless it could be admitted that the holes are formed after the production of the shell-covering on the expanded pseudopodia. But this last is clearly a single act, and its plan is evidently not such as would be adopted if the protection of sarcode were the object in view, the subdivision into many projecting branches most delicate and fragile at their points exposing it as much as possible to every injury; and therefore presenting a form and arrangement not at all likely to promote the comfort and convenience of the animal if it were to exist long in that state; and when to this we add that the pseudopodia, which are the means by which the foraminifera communicate with the external world, are sheathed by their shell-covering so as to be incapable of action, and moreover that every part of the animal becomes completely enclosed, the conclusion seems inevitable that this is not a condition in which it passes any considerable portion of its life, but that it is, as already suggested, merely the closing and final act. The holes through the thick shell however present a different history, they show by the quantity of shell-material removed and by the way in which separate holes have run together, that time has been spent in their formation, and they have also a clear and intelligible use in the economy of the animal, this being to open free communications between the internal and external sarcode. As to the process by which the shell-matter is removed it seems impossible under the circumstances to suppose it done in any other way than by

absorption by the sarcode in contact with it. Among the specimens shown is one of P. tubulosa which has been completely broken open and shows that the process of absorption is not confined to the outer walls but that the inner partitions which at first formed parts of the walls of the separate chambers are also in great part removed, throwing the whole of the interior into one large irregular cavity.

The quantity of carbonate of lime deposited at once in the covering of the external sarcode and its pseudopodia is so considerable that some unusual source might naturally be looked for to supply it, and this is apparently found in the shell-material redissolved by the process just described, which must eventually lead to the sarcode being excessively charged with mineral matter and may be considered a sufficient reason for the final catastrophe; and if the view here given of the later stages of the life of Polymorphina tubulosa be correct it adds another point of interest by showing that the deposit of shell-material, in this one case at least, is more of a chemical than a vital act.

Ordinary Meeting, February 5th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Commander Alexandre Cialdi, of Rome, and Dr. Edward Schönfeld, Director of the Mannheim Observatory, were elected Corresponding Members of the Society.

Mr. BAXENDELL read the following letter from Sir J. F. W. HERSCHEL, Bart., M.A., F.R.S., &c., dated Collingwood, February 2, 1867:—

"May I be permitted to offer a remark to the Lit. and Phil. Society on the very striking fact mentioned in Monsr. Buys Ballot's letter on storm warnings in the report of the last meeting of the Society ('Proceedings,' vol. vi. p. 83)—where, speaking of the gales of wind that accompany great barometric fluctuations he observes: '*It is a fact above all doubt* that the wind that comes is nearly at right angles to the line between the places of highest and lowest barometer readings. The wind has the point of lowest height on its left hand.'

"In my paper in 'Good Words' on the weather and weather prophets, published in January, 1864, (and written in November, 1863) the fact of this perpendicularity of direction of the wind to that of the wave's advance, in the case of the hurricanes which accompany the 'great November wave' is noticed; and in a few words added to the

original notice of this peculiarity, in a reprint of that paper which has lately appeared ('Familiar Lectures on Scientific Subjects,' p. 144, note), an analogy is suggested between the fact in question and the transverse rotation of the etherial molecules in the propagation of a circularly polarized ray of light.

"The remarkable feature in question as it now appears from this statement of Monsr. Buys Ballot, is no special peculiarity of the November gales, but a general one. And as such, an origin has to be sought for it in the dynamical laws which connect the movements of the individual aerial particles (which constitutes the wind) with the direction and progress of the *advancing form* which constitutes the atmospheric wave.

"Weber, in his 'Wellenlehre,' has made us familiar with the conception of the propagation of a wave by the successive transfer from particle to particle of circular or elliptic movements. In the case of undulating liquids—to which his work chiefly refers, the plane of such circulation coincides with the *line of advance* of the wave, and thus are easily explained all the phænomena of breakers and (as I have since shown) of alternating tide currents. But there is nothing, dynamically speaking, to limit such circulating movements to that plane. It is equally conceivable that their plane may be obliquely situated: and that a wave *may* be propagated even in the extreme case of the perpendicularity of the two directions, the phœnomena of polarized light alluded to, demonstrate. I cannot help thinking then that a *general feature* of this nature being now so strikingly brought before us, there is something more than a mere resemblance in the phœnomena I have thus ventured to assimilate and that the attention of hydrodynamicians might be advantageously directed to the subject as likely to cast some additional light on the mechanism of storms.

"P.S.—Would it not be worth while to enquire whether

the condition as to the wind having the barometric minimum (the trough of the wave) on its left hand is not reversed in the southern hemisphere ?"

Mr. Eddowes Bowman, M.A., exhibited and explained the construction and peculiarities of Hoffman's Polari-Microscope.

PHYSICAL AND MATHEMATICAL SECTION.

January 31st, 1867.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

"Results of Raingauge, Anemometer, and Ozone Observations, made at Eccles, near Manchester, during the year 1866," by Thomas Mackereth, F.R.A.S., F.M.S.

The following results are intended as a sequence to those presented to this section last year. I regret, however, that on account of the removal of my observatory, some of the observations were interrupted during a period of three months. The observations to the end of April were made under the same circumstances as those of last year, an account of which will be found p. 107, vol. 5, of the Proceedings of this Society. The rainfall 3ft. from the ground continued to be measured under former circumstances till my removal was completed in August, when all the observations were resumed as before. The rain is measured from three different gauges. Two of the gauges stand close to each other in the same plane 3 feet from the ground, 75 feet from my house, and 21 feet from my astronomical observatory. The receivers of the rain gauges on the ground are of different shapes, one being round, 10 inches in diameter, the other 5 inches square, in both the edges are turned inward. The other gauge is now 34 feet from the ground and 4 feet above the ridge of my house—and free from every obstruction.—Up to April, at my residence, it was 32 feet from the ground. The receiver is a square one of the

same size and shape as the 5-inch square one 3 feet from the ground.

First I represent the rainfall for 1866, as measured by the 10-inch gauge 3 feet above the ground, and 145 feet above the sea. Compared with the average fall for six years at Eccles:—

Quarterly Periods.		1866.	Fall in Inches.	Average of 6 Years	Differences.	Quarterly Periods.	
1865.	1866.					1865.	1866.
Days	Days						
50	64	January	3·162	2·483	+0·679	7·162	8·441
		February	3·122	2·193	+0·929		
		March	2·157	2·742	−0·585		
38	39	April	0·361	1·562	−1·201	4·929	6·336
		May	1·702	2·314	−0·612		
		June	4·273	3·129	+1·144		
40	68	July	4·086	2·973	+1·113	7·633	15·929
		August	4·584	3·566	+1·018		
		September	7·259	4·345	+2·914		
49	61	October	2·606	3·447	−0·841	8·085	12·370
		November	5·758	3·586	+2·172		
		December	4·006	2·528	+1·478		
177	232		43·076	34·868	+8·208	27·809	43·076

The above table appears to add further confirmation to the remarks made on the rainfall at Old Trafford by Mr. G. V. Vernon, F.R.A.S., F.M.S. at the last meeting of this section. There is perhaps one thing worthy of notice, viz., that the average annual difference of rainfall between Old Trafford and Eccles for 6 years is about half an inch. This year the difference is not quite one tenth of an inch. The difference between the height of my former station above the sea and my present one is 27 feet. This probably may account for my having measured a trifle more rain during the last five months.

I now present the monthly amounts that fell into each gauge, and the number of miles of the horizontal movement of the air.

1866.	Rainfall in 10 inch round gauge, 3 feet from ground	Rainfall in 5 inch square gauge, 3 feet from ground.	Rainfall in 5 inch square gauge, 34 feet from ground.	Amount of horizontal movement of the air, in miles.
January.....	3·162	3·215	3·103	5,794
February	3·122	3·079	3·056	4,664
March	2·157	2·087	1·870	4,573
April	0·361	0·299	0·307	5,833
May	1·702	1·655	,,	,,
June	4·273	4·284	,,	,,
July	4·086	4·003	,,	,,
August	4·584	4·521	4·060	5,333
September.....	7·259	7·230	6·625	5,325
October.....	2·606	2·585	2·314	3,759
November.....	5·758	5·585	4·938	5,860
December	4·006	3·922	3·012	5,753
Totals	43·076	42·465		
Taking out May, June, and July, in 1st and 2nd columns—Totals are.....	33·015	32·523	29·285	46,894

The next table shows the relative proportion of the winds for each month reduced to the four cardinal points, the number of days on which rain fell, together with the average amount received in each gauge.—

1866.	Relative Proportion of Winds to the Days of each Month.				Days on which Rain fell.	Average Amount of Rain received in each Gauge.		
	N.	E.	S.	W.		10 inch Round Gauge, 3 feet from Ground.	5 inch Square Gauge, 3 feet from Ground.	5 inch Square Gauge, 34 feet from Ground.
January.....	3	2	14	12	20	0·158	0·160	0·155
February ...	7	3	9	9	22	0·142	0·139	0·138
March	9	6	9	7	22	0·098	0·094	0·085
April	8	10	7	5	9	0·040	0·033	0·034
May	7	9	3	12	11	0·154	0·150	,,
June	4	8	9	9	19	0·224	0·225	,,
July	6	7	6	12	13	0·314	0·307	,,
August	10	4	4	13	26	0·175	0·173	0·156
September...	4	4	10	12	29	0·250	0·249	0·227
October.....	11	11	3	6	14	0·186	0·184	0·165
November ...	7	2	5	16	22	0·261	0·253	0·224
December ...	6	3	8	14	25	0·160	0·156	0·120
	82	69	87	127	232	0·180	0·177	0·145

If the direction of the wind from the above table be compared with the monthly rainfall in the first table, it will be seen that the fall is always in excess when the west and south winds prevail over the north and east; and that if

the average rainfall, and the number of days on which rain fell in each month, be compared with the general direction of the wind, it will be seen that the prevalence of the south and west winds, not only increases the amount of rainfall, but the number of wet days. The excess of rainfall on the ground over that at 34 feet from the ground appears also to be affected by the wind, for the excess on the ground increases when south and west winds prevail over the north and east. The only real exceptions being in the difference of rainfall 34 feet from the ground in March, and in the number of days in July. I present below a table of comparison illustrative of these remarks.—

1866.	Ratio of Winds. N. and E. to S. and W.	Differences from the Average No. of Days of Rainfall.	Differences of Rainfall from the Average of 6 Years. In 10 inch Round Gauge, 3 feet from Ground.	Differences of Rainfall from the Average of 6 Years. In 5 inch Square Gauge, 3 feet from Ground.	Differences between Rainfall 3 feet from Ground, and 34 feet from Ground.
January	5 to 26	+3	+0.679	+0·732	—·112
February.....	10 to 18	+6	+0·929	+0·886	—·023
March	15 to 16	+4	—0·585	+0·655	—·217
April	18 to 12	—4	—1·201	—1·263	—·008
May.....	16 to 15	—4	—0·612	—0·659	,,
June	12 to 18	+1	+1·144	+1·155	,,
July.....	13 to 18	—3	+1·113	+1·030	,,
August.....	14 to 17	+6	+1·018	+0·955	—·461
September ...	8 to 22	+11	+2·914	+2·885	—·605
October	22 to 9	—5	—0·841	—0·862	—·271
November ...	9 to 21	+5	+2·172	+1·999	—·677
December ...	9 to 22	+7	+1·478	+1·394	—·910

The next table shows the average daily fall in each kind of gauge, when the velocity of the wind has ranged between the number of miles indicated in the first column.—

Daily Movement of Wind.	34 feet from the Ground. 5 inch Square Gauge.	3 feet from the Ground. 5 inch Square Gauge.	3 feet from the Ground. 10 inch Round Gauge.
0 to 50 miles	·054	·050	·053
50 to 100 ,,	·088	·095	·098
100 to 150 ,,	·098	·107	·110
150 to 100 ,,	·191	·213	·217
200 to 250 ,,	·233	·250	·251
250 to 300 ,,	·150	·195	·202
Above 300 ,,	·212	·228	·228

I have a few remarks respecting the large amount of ozone I now detect over the quantity I detected at my former station. My present station is only 27 feet higher than my former, and about one-third of a mile farther north. I was before about one-third of a mile from the most populous part of the village. I am now nearly two-thirds of a mile from it. I have used similar test-papers at both stations. I make my own tests, and have used those which were made at the same time at both stations. The average amount I used to detect at my former station from August to December, was = 0·79 on the scale of comparison. Last year from August to December, the amount was = 2·40 on the scale. Previous to my removal I tried a test-paper at each station. I found each comparison average a similar difference. The amount detected by Mr. Vernon, at Old Trafford, was usually a little in excess of mine for the months named; but the last year shows a very different result. The average amount detected by Mr. Vernon from August to December of last year (see the Manchester and Salford Sanitary Association's Weekly Returns), was 0·91 on the scale; showing a difference from my present station of 1·49 or nearly 1½, while the amount is 1·61 above the amount detected at my former station.

Observations of the Occultation of Aldebaran by the Moon, January 16th, 1867.

By J. Baxendell, at Mr. Worthington's Observatory.

53° 30′ 50·0″ N. 8m. 56·16s. W.

	Local Sid. Time.	Greenwich Mean Time.
Disappearance	2h. 59m. 6·1s.	7h. 25m. 0·2s.
Reappearance	4 10 7·8	8 35 50·2

The disappearance and reappearance were both instantaneous, and no unusual phenomenon was observed.

The telescope used was the equatorially mounted achromatic of 70 inches focal length and 5 inches aperture with a power of 68.

By A. Brothers, at 110, Upper Brook Street, Manchester.

53° 27′ 56·0″ N. 8m. 54·38s. W.

Disappearance	2h. 59m. 6s.	Local Sid. Time.
Reappearance	4 10 13	„ „

The disappearance was instantaneous. The reappearance was not so well observed, as the star must have emerged a few seconds before it was seen. The telescope is a refractor equatorially mounted, and in other respects identical with the one employed by Mr. Baxendell. Power used, 88.

"On the Elements of the Variable Star R Persei," by Joseph Baxendell, F.R.A.S.

In Professor Schönfeld's "Results of Observations of Variable Stars, &c.," read at the last meeting of the Section it is stated, "that the period of R Persei is about 205·5 days, and that one period must be added to the number hitherto assumed to have happened since the date of Bessel's observations." This value of the period is derived from the following four observed maxima compared with Bessel's single observation of the star, when at or near a maximum on the 6th of January, 1833.—

1865, March 12.
„ October 6.
1866, April 27.
„ November 20.

R Persei has been under pretty regular observation at Mr. Worthington's Observatory since the announcement of the discovery of its variability by Dr. Schönfeld in September, 1861, and a projection of all the observations has

yielded the epochs of eight maxima, or double the number observed by Schönfeld, thus enabling me to determine with greater exactness the length of the mean period. These maxima are as follows:—

1861, October 16	8·7 mag.
1862, November 21	8·9 „
1864, January 8	9·3 „
„ August 20	8·7 „
1865, March 9	8·4 „
„ October 1	8·7 „
1866, April 19	8·2 „
„ November 14	9·0 „

Equating, and treating by the method of least squares, we have

Mean period = 206·8 days.
Mean epoch = 1864, August 10·7.

Calculating from these elements we find that a maximum ought to have occurred 1832, November 25, or 42 days earlier than Bessel's observation. As this difference considerably exceeds any that could be due to the form of the light curve at its maximum, it seems probable that the length of the period has slightly increased since the date of Bessel's observation.

The differences between the observed and the calculated times of maxima are:

Observed—Calculated.	Observed—Calculated.
+ 4·7 days	+ 3·9 days
—7·9 „	+ 3·1 „
—8·5 „	—3·7 „
+ 9·7 „	—1·5 „

The mean magnitude of the star at maximum appears from my observations to be 8·7. The magnitude at minimum is generally below 13.

Comparing Dr. Schönfeld's times of the last four maxima with my own, it will be seen that in every case his date is later than mine, the differences being

Schönfeld—Baxendell.
+ 3 days
+ 5 days
+ 8 days
+ 6 days

the mean difference being 5·5 days. At present I am unable to explain the cause of this difference, but hope that future observations specially directed to the point will clear up the mystery.

The next maximum of R Persei will be due 1867, June 10.

Mr. Baxendell read the following extract of a letter from George Knott, Esq., F.R.A.S., dated Woodcroft Observatory, January 24th, 1867, and exhibited the Light-curve to which it refers :—

"I have much pleasure in sending a copy of my light-curve of R Vulpeculæ for 1866. In forming the November maximum I have availed myself of a few observations kindly sent me by yourself. With the exception of a marked discordance at the end of the month our estimates agree very well, and show that the November maximum was an unusually bright one — brighter in fact than I have ever observed it before. But what I wish particularly to call your attention to, is the curious flattening of the ascending curve. I have noticed traces of this peculiarity in several instances in the case of this star, and cannot help regarding it as a real feature, though I should like to establish this view by further observation before laying too much stress upon it. You will notice, too, the curious flattening of the curve at the September minimum, which was a bright one."

Maxima and Minima of R Vulpeculæ, 1866.

Maxima.		Minima.	
DATE.	MAG.	DATE.	MAG.
July 7·0	7·6.	May 2·5.	13·2.
Nov. 15·5	7·1.	Sept. 13·5.	12·3.

Ordinary Meeting, February 19th, 1867.

J. P. Joule, LL.D., F.R.S., &c., Vice-President, in the Chair.

Mr. Sidebotham inquired whether any member was acquainted with the particulars of a remarkable discovery in connection with the conversion of dynamical into electrical force, by M. Siemens and Dr. Wheatstone, a brief notice of which appeared in the *Athenæum* of Feb. 16.

Mr. Wilde said, that he happened to be present at the meeting of the Royal Society on the 14th inst., when M. Siemens' interesting paper "On the Conversion of Dynamical into Electrical Force, without the aid of Permanent Magnetism," was read. The apparatus used in his experiments, and exhibited at the meeting, consisted of two electro-magnets, coiled with long and thin wires, and between the poles of these electro-magnets an armature, also coiled with long and thin wires, was made to revolve rapidly by means of pulleys.

When a voltaic battery was connected with the wires of the electro-magnet for a few moments, and afterwards disconnected, the residual, or *permanent*, magnetism of the electro-magnets was sufficiently strong to generate a feeble current of electricity in the wire of the revolving armature. The feeble current thus generated was then transmitted through the coils of its own exciting electro-magnet, instead of the current from the battery, and by the mutual action and reaction of the electro-magnet and the armature upon each other, the magnetism of the machine could be exalted to such a degree as to require the application of very considerable power to drive the apparatus.

That this increase of magnetism was attended by an increase of dynamic electricity in the wires surrounding the armature and electro-magnet was manifest from the brilliancy of the spark at the commutator and the increased deflection of a galvanometer needle placed within the influence of the electro-magnetic circuit; but the quantity of electricity was not sufficient to ignite wires; and was comparatively small in proportion to the mechanical force expended in producing it.

The apparatus used by Dr. Wheatstone in his experiments, which was also shown in operation at the meeting, was very similar in construction, and identical in principle, with that of Siemens', though each of these experimentalists appears to have worked out his results independently.

Instead of the long and thin wire used by Siemens in his machine, Wheatstone employed coils of thicker wires, and the wire on the electro-magnet was of the same diameter, and about eight times the length of that on the armature. The coils of the electro-magnet had consequently about eight times the resistance of the armature circuit. In this arrangement, the wire surrounding the armature being the generating circuit, it follows from a well-known property of the electric circuit, that the amount of dynamic, or useful effect, which the current is capable of exerting when the resistance of the coils of the electro-magnets is in circuit, is about one-ninth of the total amount which the armature coil is capable of evolving when the electro-magnet is excited to the same degree by a separate battery or other electro-motor.

This peculiarity of the machine was made evident by Dr. Wheatstone in a very striking manner by means of the following well-devised experiment:—Two branch wires were led off from the extremities of the armature coil, and when the electro-magnet was fully excited the free ends of the branch wires were bridged across by means of a fine

platinum wire about nine inches in length, which wire immediately became white-hot, and as suddenly ceased to be visibly hot until contact with the branch wires had been broken for a short time and afterwards re-established.

This momentary ignition of the platinum wire appeared to Mr. Wilde to be due to the diversion of the greater portion of the current generated by the armature into the new channel or short circuit established by the platinum wire through the armature coils, before the high degree of magnetism which had been maintained in the electro-magnet by the full current from the armature had been reduced by the loss of a portion of the current so diverted.

The results obtained by Siemens and Wheatstone are undoubtedly very interesting from a scientific point of view, but, practically, the exciting of the electro-magnet by the current from the armature used in connection with it, produces results very inferior to what are obtained from similar machines, the electro-magnets of which are excited by a separate source of electricity of small power, as, in the latter case, the full force of the current generated by the armature coil is available for the performance of external work.

Mr. Wilde having himself, some time ago, made some experiments similar to those of M. Siemens and Dr. Wheatstone, came to the conclusion that in the present state of our electrical knowledge, the difficulty of utilising the current from the armature after it had passed through the coils of the electro-magnet was insuperable; as it is absolutely necessary that the coils of the electro-magnets of electro-magnetic machines, when excited by intermittent currents, should be of considerable length, in order that they may acquire and retain a charge of electricity or magnetism, as has been shown by Drs. Henry and Faraday in their experiments on the induction of a current upon itself. On the other hand, for the production of powerful electro-dynamic effects, it is necessary that the wire of the armature coil

should be comparatively short and thick. Hence the incompatibility of the armature and electro-magnetic circuits, when exalted dynamic effects, outside the machine, are required.

It is not a little interesting to observe how, in the progress of physical science, the same train of reasoning is often pursued by several individuals having no communication with one another. In the present instance, it would appear that the "augmentation of the power of a magnet by induction currents produced thereby, and reacting upon the magnet itself," has also suggested itself to other experimentalists besides M. Siemens and Dr. Wheatstone. In a letter published in *The Engineer* of July 20th, 1866, p. 42, the writer (Mr. J. Murray) says:—"The description in recent numbers of *The Engineer* of Mr. Wilde's admirable improvements in the magneto-electric machine, induces me to point out, if you will permit it through your columns, a variety of the principles embodied in his machine, which is so obvious that it cannot fail to be hit upon by some inventor before long."

It is briefly this:—"Whereas Mr. Wilde, beginning with an ordinary magneto-electric machine, uses the current obtained from it to charge a powerful electro-magnet, and from this obtains a second and more powerful current, which, used in like manner, produces one still more intense, I, using only a single machine, pass the current obtained from its armatures through wires coiled round the permanent magnets, in such a direction as to intensify their magnetism, which, in its turn, reacts upon the armatures and intensifies the current.

"It is obvious that the amount of mechanical force which can thus be converted into electricity is only limited by the capacity of the iron for magnetism; just as the power of a steam engine is only limited by the capacity of the boiler for absorbing heat."

Mr. Wilde has also received a letter on the subject of his recent experiments in magnetism from Mr. Moses G. Farmer, of Salem, Mass., United States, dated November 9th, 1866, in which he says that he had obtained an increase of 31 per cent. in the power of a magneto-electric machine, by transmitting the current from the armature through coils of wire surrounding pieces of soft iron forming the prolonged extremities of the permanent magnets of the machine. Mr. Farmer, in the same letter, adds:—"I have built a small machine in which a current from the thermo battery excites the electro-magnet of your machine to start it, and after the machine is in action, a branch from the current of the magneto passes through its own electro-magnet, and this supplies the magnetism required. It is not exactly like a person standing in a basket and trying to lift himself—because the electricity proceeds from the conversion of the mechanical energy, which must be continually supplied. Neither can it in any wise be likened to the various schemes for producing perpetual motion; but depends on the principle, that the actual energy of the mechanical force, conjointly with the potential energy of the magnet, can develope a greater amount of potential energy than is originally resident in the magnet, or, in other words, it is a method of converting part of the actual energy of the prime mover into the potential energy of magnetism."

Mr. W. L. Dickinson, having discovered an error in his calculation of the Occultation of Aldebaran (see Proceedings, December 11th, 1866, page 47), communicated the following results of the corrected computation.

Calculation for the Observatory of Robert Worthington, Esq., F.R.A.S., Crumpsall, near Manchester, Lat. 53° 30′ 50·0″ N., Long. $0^h\ 8^m\ 56{\cdot}16^s$ W.

The Occultation of Aldebaran by the Moon, January 16, 1867:—

	Sidereal Time at Observatory.	Mean Time at Observatory.	Mean Time at Greenwich.	Angle from N.Point.	Angle from Vertex.
Disappearance...	$2^h\ 59^m\ 13^s$	$7^h\ 16^m\ 10^s$	$7^h\ 25^m\ 6^s$	100°	82°
Reappearance ...	$4^h\ 10^m\ 5^s$	$8^h\ 26^m\ 50^s$	$8^h\ 35^m\ 46^s$	282°	280°

The angles are reckoned towards the right hand round the circumference of the Moon's image as seen in an inverting telescope.

Mr. Baxendell read the following letter, addressed by T. H. Babington, Esq., to G. V. Vernon, F.R.A.S., and dated Langley, 13th Feb., 1867 : —

You have been kind enough to send me a copy of a letter recently received by yourself from Dr. Buys Ballot, of Utrecht, in which the writer claims for himself the origination of the system of issuing storm warnings.

He says, "I laid down my principles before the Dutch Academy of Sciences in October, 1857; in the *Comptes Rendus de l'Académie des Sciences de Paris* in November, 1857. I made my first proposal to the Dutch Government on the 14th of November, 1859, and the system was commenced on the 1st of June, 1860, or nine months sooner than in England."

It is no doubt the fact that Admiral Fitzroy did not give his first "warning" before the 5th of February, 1861, but he had been *for years* engaged in organising the service and maturing his plans.

I feel it therefore incumbent upon me, in justice to the memory of my late chief, to offer a few remarks upon that portion of Dr. B. Ballot's letter which I have quoted.

What the nature of the system of warnings which Dr. B. Ballot claims to have introduced at Utrecht in 1860 may have been I do not know, — but Holland must, at that period, have been mainly or entirely dependent upon obser-

vations made at her own stations, that is, over an area *not larger than Wales,* and forming part of a great continent.

It will be clear to you and to every meteorologist that forecasts founded upon barometrical differences over an area so limited, and geographically so situated, could not be of much practical value,—and that for purposes of forecasting, Dr. B. Ballot could, at that period, have had but little advantage over any individual and isolated observer.

But however that may be, I will, with your permission, relate as simply as possible what I know with respect to the origination of the storm-signals — observing that any knowledge I may possess on the subject is derived from the fact that I was Admiral Fitzroy's assistant from the first establishment of the Meteorological Department in January, 1855, that I was in daily personal communication with him, and I believe, saw the whole of his correspondence.

By continued and consecutive series of charts constructed on the synoptic or synchronous principle, during the winter of 1856-57, it appeared to Admiral Fitzroy that a light was thrown on the atmospheric changes over the British Isles and their vicinity which had been unattainable previously. Those charts were prepared by myself under Admiral Fitzroy's direction, with the express view (as I know from frequent conversations with him on the subject) of gaining by their intercomparison an insight into the laws of our atmosphere which might enable us to know what weather was likely to prevail during the next two or three days, and, as a corollary, when a storm was likely to occur. Those charts however were not found sufficient by themselves, and led to subsequent arrangements and correspondence.

In the Report of the Meteorological Department in March, 1857, Admiral Fitzroy wrote — "It has been desired that a great many observations should be compared throughout the British Isles (with their neighbouring coasts and seas) at certain remarkable periods, to obtain the means of deline-

ating or mapping the atmosphere at successive times; and *thence to deduce the order of those changes of wind and weather* which affect navigation and fisheries especially."

In September, 1859 — or *two months before* Dr. Buys Ballot appears to have made his first proposal on the subject to the Dutch Government — the British Association for the Advancement of Science met at Aberdeen under the presidency of the Prince Consort, and, the question being brought before them, it was then resolved by their Council that application should be made to Her Majesty's Government for an organisation and trial of a plan by which the approach of storms might be telegraphed to distant localities.

At two meetings in Buckingham Palace early the following year (1860), minutes were authorised on this subject, and correspondence ensued which resulted in establishing a telegraphic communication of meteorological facts between twenty home stations, besides foreign ones.

When the British Association met at Oxford in July, 1860, a paper was read by Admiral Fitzroy on the measures proposed for meteorological telegraphy, which obtained approval without eliciting any opposition.

Advancing gradually, the first cautionary or storm-warning signals were made early in 1861 — on the 5th and 6th of February.

In further confirmation of the hitherto undisputed fact that it is to the zeal and energy of Admiral Fitzroy (and those only who had the advantage of his intimate acquaintance know how great and vexatious was the opposition that his plans encountered from certain quarters) that England, and to a great extent Europe generally, is or was indebted for her system of cautionary signals, I will merely quote the following paragraph from Sir Henry James's "Instructions for taking Meteorological Observations," *published in 1860.*

Sir Henry James observes — "It is unnecessary to point out the vast importance of being able to foretell the advent

of a storm many hours before it could arrive at any of our ports, and Admiral Fitzroy, impressed with the idea that this can be done by the aid of the telegraph, has *for some years past* urged upon the Government the desirability of establishing telegraphic communications daily between our most distant ports, and especially from those in the south of Ireland."

I have no desire to detract in any way from the important services rendered to meteorology by Dr. B. Ballot. When, however, Admiral Fitzroy at length succeeded in extorting from those in authority a reluctant consent to the issue of storm warnings in England, he was distinctly told that the cautions must be given on his sole responsibility and that upon him must rest the responsibility of their failure. He did not shrink from that responsibility. The storm signals are generally admitted to have been a great public benefit. Had it been otherwise, upon Admiral Fitzroy would have fallen the *dis*credit which usually attaches to failure. It behoves his friends, therefore, to take care that his name be not deprived of any of the credit which belongs to success.

Mr. BAXENDELL explained the construction and mode of using the weather signal recently invented by Dr. Buys Ballot, and also stated that the Doctor had lately found that the discovery of the fact that the direction of the wind was generally at right angles to that of a line joining the areas of high and low barometer was due to Dr. Lloyd of Dublin; but he still claimed to have been the first to make practical application of this discovery.

Dr. JOULE, F.R.S., suggested the desirability of adding to Dr. Ballot's instrument an arrow fixed in a direction perpendicular to the plane of the indicating board, with the view to prevent the possibility of any mistake being made as to the direction in which the wind might be expected to come.

Mr. Baxendell, referring to the statement made by the President of the Board of Trade in the House of Commons on the 15th instant, in reply to the questions put by Colonel Sykes respecting storm signals, remarked that a concession had certainly been made by the Government, but unfortunately it had this very objectionable feature, that while it would be far less useful than the thing asked for, it would involve a very much greater current expense. "The information which was collected daily by the scientific committee would be telegraphed as heretofore to the different ports and stations throughout the kingdom in time to allow of forecasts to be made in any place where there was a disposition to make them." But the cost of daily telegraphing all this information to the various outports and stations would obviously vastly exceed that of occasionally transmitting telegraphic notices of approaching storms; and moreover, it seems never to have occurred to the scientific committee nor to the Board of Trade that it would require scientific meteorologists at all the ports and stations to interpret the indications afforded by this crude and undigested information, men, in fact, with ability to do that which the scientific committee shrink from attempting themselves. But such men are not to be found at many of the outports, and especially at the fishing stations; and even if they were, it is not to be expected that they would voluntarily undertake onerous duties which ought to be discharged by a competent public officer appointed by the Board of Trade. The scientific committee are unwilling to issue storm warnings because they are not prepared with a system of forecasting founded on a strictly scientific basis; but this is no valid reason for allowing them to abolish the late system, which was working so well for the interests of science, commerce, and navigation, until they are prepared with something better to replace it. And it must be borne in mind that it will not in the end conduce to the interests of science, if the

public find that a body of scientific men, invested with an unusual power, are either unable or unwilling to use it for the public good.

"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes," Part III., by JAMES NASMYTH, Esq., C. E., Corresponding Member of the Society.

The author in this part of his paper describes in considerable detail the methods he employs in casting, grinding, polishing, and giving perfectly flat surfaces to the plane mirrors of reflecting telescopes.

PHOTOGRAPHICAL SECTION.

February 20th, 1867.

Dr. J. P. JOULE, F.R.S., &c., Vice-President of the Section, in the Chair.

"On the Reversed Action of Light in Photography," by JOSEPH SIDEBOTHAM, Esq.

Those who have for many years been engaged in photographic pursuits, either as amateurs or professionals, have (especially when trying some new process) met occasionally with very curious results—failures as photographic pictures—but of the greatest value as remarkable results of the effects of light. These were probably much more frequent in the early days of photography, when we were dependent in a great measure on our own manufacture of chemicals, even to the preparation of æther. Unfortunately but few of these curiosities, as we may call them, have been preserved; in the desire to produce good pictures these have been thrown aside, and too often the causes of the strange results have not been ascertained. This is the more to be regretted as discoveries of great value may be hidden under these so called failures. On a former occasion I exhibited here a collodion positive, on glass, of a landscape, with the natural colours, red, green, and blue, very fairly represented, although much duller than in nature. This was an over exposed picture, and not noticed particularly until a day or two after it was taken, and all my subsequent attempts were unsuccessful to obtain another similar picture.

It is well known that when plates are over exposed in the camera the action of light becomes reversed; thus, a sky which ought to be black becomes transparent, and a print from this looks somewhat like a negative on paper. Still the action is by no means regular, and there are many points connected with this action which require careful investigation.

It is my intention to bring some of these before you this evening in a brief manner, and to exhibit some curious specimens in illustration.

All photographers must have noticed now and then where two lines of light and dark meet, that the light is rendered more intense just at the edge of the black or dark object; in portraits of gentlemen this is sometimes very prominent, the dark figure is thrown up as intended from the light background, but the latter is so much more intense just round the figure that it has sometimes the appearance of light streaming from behind. This effect is sometimes seen in landscapes — round a tower or even a tree projected against the sky runs a line of intense white much deeper than the sky itself. On the other hand it sometimes happens that in a landscape with mountains the edge against the sky seems to be robbed as it were of a portion of its intensity, and so when printed exhibits a dark edge.

Again, it is well known that the sky, seen through small openings in trees or small windows in a ruin, is much more intense in a negative than the great mass of the sky; also that in taking an interior view, where light streams through a window in front of the camera, the effect produced on the negative is as if the light came round, illuminating to some extent the inside, and gradually as it were eating away the tracery of the window.

In some cases where the bright sky is seen through an opening of the trees, the trees and branches around the opening are converted in the photograph into pure white

tracery on a darkened sky, and the remainder of the picture quite natural; in some cases again, one half of a photograph may be correct as a positive, and the other, the distance exactly like a paper negative. I bring with me for exhibition specimens of these several effects, which appear intimately connected together, and deserving of our attention.

The question for us to consider is whether these effects are to be accounted for altogether by the received theories of the reversed action of light on iodized plates or paper when exposed too long in the camera, in other words, what used to be called solarization, or whether to some extent, at least, these are not copies of the natural effects as the camera sees them, and as we should see them, if we looked aright.

In Tyndall's work on the glaciers of the Alps is a reference to a curious phenomenon he saw, where the trees and shrubs projected against the sky showed as brilliant white, he says this phenomenon is rarely seen by guides or travellers, perhaps because they avoid looking towards the brilliant sky; he also quotes a letter from Professor Necher to Sir David Brewster, describing the same phenomenon. He says he has seen it at different times of the day, and says that the appearance of the trees projected against the sky is as if they were made most delicately of the purest silver.— At pages 180 and 181 are sketches, which, as you will see, are wonderfully like the photographs I have to show.

That some of these effects can be accounted for I am well aware, but when taken together there is much that is inexplicable, the explanation for one case will not suit another. I would, therefore, make the suggestion, that in some states of the atmosphere, these bright lines are to be found in nature, either visible, or of such actinic power as to produce the results you see; there are certain effects in nature, for instance, the streaming light of the

sun from behind clouds, of which the camera takes no notice, and on the other hand, there may be effects in nature invisible to us, but which act like the invisible rays of the spectrum with great chemical intensity.

The following photographs were exhibited to illustrate the above remarks:—

No. 1. Showing white lines round figures, and chair in carte de visite portrait.

No. 2. Showing white line round church tower and yew tree.

No. 3. Showing white line faintly above the hill, also the increased depth of shade at the extreme top of it.

No. 4. Showing intense white line dividing landscape from sky, also the railings on bridge against the sky showing intense white, whilst those against the trees are black.

No. 5. Curious compound of positive and negative, the house, road, trees, &c., in foreground being positive, and correct, the sky black, and the distant trees and buildings white.

No. 6. Bridge and stream, Fountains Abbey. Showing the foliage around the opening to sky traced in pure white, the other part of photograph being natural.

No. 7. Showing the blurring of dark tracery when light streams through a window.

No. 8. Showing one of the distant hills reversed, appearing light against a dark sky, the other mountains appearing natural.

Mr. DANCER remarked that several years since he had attempted to explain the marginal appearances which some photographic negatives exhibit, by supposing that the force which governed the deposit forming the picture was more energetic at the abrupt boundary line between a light and dark object than on other parts of the exposed sensitive plate where there was less contrast, this intensity of force bearing some analogy to that which is observed on the edges

of bodies influenced by magnetic, electric, or chemical action. More recent observations had induced him to alter this opinion. He thought the phenomena were caused by interference of light accompanied by diffraction fringes of colour produced by the projection of a dark body against a bright sky. The actinic effect of these fringes might possibly be increased or decreased according to the amount of chromatic correction of the camera lens for the primary and secondary spectra. Mr. Dancer stated he had frequently noticed the reversed action of light from over exposure; with the old Daguerreotype process he had obtained very perfect negative instead of positive pictures. On one occasion, in 1840, he accidentally exposed an iodized Daguerreotype plate in the camera much longer than the usual time; in this picture the natural colours of the objects were brought out in a marked degree. This picture he exhibited to many friends, but from not being fixed it faded after some time. Subsequent attempts on the same objects failed in obtaining the natural colours.

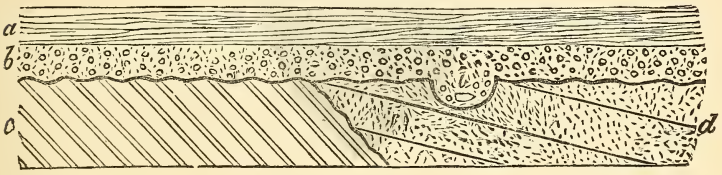
Ordinary Meeting, March 5th, 1867.

J. P. Joule, LL.D., F.R.S., &c., Vice-President, in the Chair.

Mr. Binney, F.R.S., F.G.S., said that some time since, by the kindness of one of our members (Mr. W. Mellor), he had been enabled to see an interesting section of the drift and the underlying triassic and coal measures at Ardwick. The following beds were met with in the descending order:—

	Ft.	In.
Till, or Boulder Clay, about	45	0
Lower Gravel.....	2	6
Lower Soft Sandstone (Trias)	0	0
Upper Coal Measures	0	0

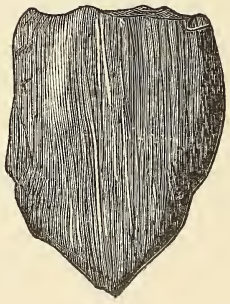
The accompanying wood cut will show the position of the Strata:—



a Till or Boulder Clay.
b Sand and Gravel.
c Upper Coal Measures.
b' Circular hole in the Sandstone filled with Sand and Gravel.
d Lower Soft Sandstone (Trias).

The gravel under the Till was quite dry, and when it was removed by tunnelling in the sandstone so as to let the whole of it fall down, the under surface of the Till showed a polished appearance, and was as smooth and even as a plastered ceiling. It was composed of granitic, trappean, silurian, and carboniferous rocks, many of the latter being

of Spirorbis limestone, all more or less rounded and mixed with a reddish-coloured sand. The thickness of it was 2 feet 6 inches, and it rested on an under surface of upper coal measures and lower soft sandstone, which bore conclusive evidence of erosion and long exposure to aqueous and atmospheric agencies. In the sandstone was a singular excavation, of a basin shape, about 2 feet deep and 2 feet 10 inches in diameter, and marked *b'* in the wood cut, filled with rounded pebbles and sand. One of the former was larger than the rest, and composed of Spirorbis limestone found in the neighbouring upper coal measures, and was of a pointed form, somewhat like the cutter of a planing machine, and rounded, polished, or striated on all sides but one. Its weight was sixteen pounds, and its form is shewn in the accompanying wood cut, about $\frac{1}{3}\frac{1}{6}$th the size of the original :—



The surfaces of rocks under the drift is generally interesting to geologists, and is now attracting considerable attention. How the hole above described was formed it is very difficult to say. It might have been made by an eddy of water carrying the stones round, and thus excavating the rock, but in that case we should have expected to find all the pebbles very nearly of a similar shape and well rounded, which is not the fact. The form of the stone herein figured, as previously stated, reminds us of a cutter, and if it were fixed at the bottom of a mass of ice moving

round in water it would very soon form a hole like that above described.

He (Mr. Binney) wished geologists to examine carefully all the surfaces of rocks *in situ* under the Till, especially those containing hollows with gravel or stones in them, which might come under their observation, and not only record the forms of the holes, but also the shape of the stones found in them.

"On some of the Conditions of Molecular Action," by Arthur Ransome, M.B., M.A., Cantab.

The scope of this paper was restricted to the action of different substances upon one another through the intervention of purely molecular forces or affinities.

The following propositions probably include what is known of molecular force :

1. That it depends essentially upon the elementary nature of bodies.
2. That it acts without regard to *mass*.
3. That it increases inversely as the distance at some enormous ratio.
4. That its action is affected by calorific, electric, and probably by luminous vibrations, and by chemical affinity.
5. That it does not seem to differ, in kind, from the attraction of cohesion, which binds together the particles of the same substance.
6. That chemical affinity may be similar in kind, but that it differs from molecular attraction in the power of producing *combination*.

When molecular operations take place between the particles of bodies of dissimilar chemical composition, the following conditions are found to favour the production of molecular and chemical changes :

(A) That two or more of the substances submitted to molecular influences have a more or less powerful attraction for one another.

(B) That their physical condition is favourable to molecular action.

(C) That the molecular agent or "catalyte" has very low *chemical* affinities for the substances acted upon.

(D) That the molecules of the catalyte are free.

Each of these conditions were considered separately in their bearing upon different molecular actions.

(A) *Of Molecular Affinity.* All substances may possess absolutely the same amount of molecular force, but from the differences in the degree of approximation of their particles, from their varying chemical affinities, and from the various extent to which they are subject to the different affections of matter, they may not be able equally to exert their molecular power.

The following classes of actions were noticed as instances of molecular affinity, and the mode in which it acts was pointed out:—

1. *Attraction of solids for liquids,* including capillary attraction; the rise of liquids in porous substances, and upon the surfaces of solids; the formation of drops; and some of the phenomena of cementation by means of colloid substances.
2. *Mixtures of fluids.*—It was pointed out that in order that two fluids should be entirely miscible, it is necessary that the particles of each fluid should have for those of the other a greater molecular affinity than they have amongst themselves.
3. *The attractions of vapours and gases* were illustrated by the distillation and diffusion of mixed vapours and gases.
4. *The solution of solids* was supposed to be due to molecular affinity, causing disintegration and liqui-

faction before mixation; and the influence of heat upon the action was considered.

5. *In catalytic actions* the force sometimes produces decomposition, at others synthesis. When contact produces decomposition, as when binoxide of manganese facilitates the evolution of oxygen from chlorate of potash, the molecular affinity of the catalyte is probably greatest for one of the constituents of the substance acted upon, and it attracts this to such an extent as to enable it to gain its freedom. When synthesis results, as in the oxidation of alcohol by means of platinum black, the catalyte draws the particles together, or it may be able to concentrate upon itself one of the elements so exclusively that it may appear in an undiluted or liquid form.
6. The phenomena of osmose were fully investigated, and were referred to the mutual attractions of the two fluids and of the septum itself.
7. Dialysis probably differs from osmose, in consequence of the peculiar molecular properties of colloid substances.
8. *The attraction of colloids, and of heated metals for gases and vapours,* was illustrated by the recent researches of Dr. Graham and Messrs. Dumas and St. Clair Deville.

(B.) *Molecular conditions favourable to catalysis.* Since molecular attraction is independent of mass, and since its power is only exerted at very small distances, it is evident that molecular attraction can only be exerted at the surfaces of the acting body, the second stratum of molecules will scarcely, if at all, affect substances lying on the surface; hence a large extent of surface is an important aid to molecular actions. Points of matter possess still greater advantages from their power of undisturbed action. These remarks were attested by observations upon catalytes, by

the power possessed by points of facilitating vaporisation and crystallisation.

(C.) *Low chemical affinities of catalytes.* When it is considered that the molecular forces exerted by a catalyte can only be employed when the substances to be acted upon are brought into very close contact with it, so close that if these substances had strong chemical affinities for one another they would combine; and when, further, it is considered that if the catalyte united chemically with other material it would soon cease to exist as such and would lose its catalytic power, it becomes obvious that catalytes must be chemically indifferent to the substances they act upon, and not only is this the case, but all the most energetic catalytes display chemical indifference not only towards those substances which they affect, but also towards ordinary chemical reagents; instances of this fact were found in the catalytic properties amongst metalloids, of charcoal, glass, and sand; amongst metals, of platinum, and others which are least affected by oxygen. The researches of Dr. Charles Henry and Mr. J. A. Ransome, and of the Rev. J. Eyre Ashby, upon the catalytic powers of metals and their oxides, were quoted, and the conclusion was drawn that it is not those oxides which have an excess of oxygen, nor yet those which most readily part with it, but it is the sesquioxides whose influence is most felt. It was also pointed out that ferments, the most remarkable substances in the organic kingdom for their catalytic power, belong universally to the class of *colloid* bodies, one of whose chief characteristics is that of "chemical indifference."

(D) The molecules of the catalyte must be free from any stain of vapour or any other substance which might partially impede its molecular action. The condensation of vapour upon solids had been noticed by Professor Magnus and Messrs. Janion and Bertrand, and the importance of cleansing catalytes by heat or other means was pointed out; also the

effect of acids upon alloys and the energy of nascent substances was ascribed in part to their freedom from all taint of other matter.

Catalytic bodies were shown to fulfil all these conditions, but it was pointed out that many forms of so-called catalysis were complex, not simple processes.

The application of these facts to physiology was remarked —the presence in the living body of substances whose chemical reactions are continually modified by variations in their molecular condition, and by which energy was stored up until it is required, and by which, probably, fermentative changes are produced.

Observations of the Eclipse of the Sun, March 6th, 1867.

By Mr. BAXENDELL, at Mr. Worthington's Observatory. 53° 30′ 50·0″ N. 8^m $56·16^s$ W.

First contact ... . 8^h 20^m $16·5^s$ a.m. G.M.T.
Last contact ..... 10^h 50^m $21·9^s$ „ „

About a minute before the time of first contact two or three small clouds passed over the sun, and its limb, which had been tolerably steady and well defined, became excessively tremulous, and continued so till after the contact had taken place. The time given may therefore be uncertain to the extent of *two* or even *three* seconds. At the end of the eclipse the limb of the sun was very steady, and the time of last contact was observed very satisfactorily. Only one small *black* spot and two or three minute ones of a penumbral character were visible on the sun's disc; but a fine group of faculæ near the following limb formed a very interesting object for examination during the early part of the progress of the eclipse.

By Mr. BROTHERS, F.R.A.S., at Upper Brook Street. 53° 27′ 56·0″ N. $8^m\ 54{\cdot}38^s$ W.

First contact ..... $8^h\ 20^m\ 15{\cdot}4^s$ a.m. G.M.T.
Last contact ..... $10^h\ 50^m\ 20{\cdot}7^s$ „ „

By Mr. MACKERETH, F.R.A.S., at Eccles. 53° 29′ 12″ N. $9^m\ 21{\cdot}5^s$ W.

First contact ..... $8^h\ 20^m\ 15^s$ a.m. G.M.T.
Last contact ..... $10^h\ 50^m\ 10^s$ „ „

Ordinary Meeting, March 19th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"Observations of the Eclipse of the Sun, March 6th, 1867," by J. B. DANCER, F.R.A.S.

Observatory, Old Manor House, Ardwick, Lat. 53° 28′ 23″ N., Lon. 8^m 53·93^s W.

First contact, not timed.
Contact with elongated black spot..... 8^h 43^m 36^s G. M. Time.
Last contact of moon's limb ..... 10 50 17·5 „

Just before the first contact my attention was unfortunately taken from the telescope, and when I got to the instrument again the eclipse had commenced.

The weather was very favourable and the atmosphere remarkably steady; clouds occasionally passed over the sun but did not obscure the disc for any length of time.

The telescope used was 10 feet 6 inches focal length and 7¼ inches diameter. The full aperture of the object glass was employed, with a power of 50, and using a diagonal glass sun prism numerous groups of faculæ were visible, but no maculæ excepting the dark spot named above. The behaviour of this spot interested me, and as it may have been noticed by other observers I will describe it. I have already stated that the atmosphere was very steady, indeed I have seldom seen the edge of the sun so free from all disturbance. When the moon's edge approached the dark spot it suddenly

became tremulous and lighter in colour before it disappeared behind the body of the moon; there was no visible cloud passing at the time and the sun's edge was sharply defined and perfectly steady.

During the progress of the eclipse the serrated edge of the moon was beautifully distinct, powers from 50 to 320 were used to look at the prominences on the moon's edge. I had several telescopes directed to the sun; some of these had $4\frac{1}{4}$ inches and $5\frac{1}{2}$ inches diameter object glasses. The value of aperture in observing the edge of the moon and the faculæ on the sun's disc was very manifest when using the $7\frac{1}{4}$ inch diameter glass.

The temperature in the sun during the eclipse was taken at intervals with a very delicate black bulb mercurial thermometer. The record shows that the increase of temperature due to the increasing altitude of the sun was greater than that of the opposite effect caused by the obscuration of the disc until nearly the time of the greatest phase. The effect of passing clouds is shown by the fluctuations of the mercury during the progress of the eclipse.

Temperature 3 feet 6 inches above ground. Black bulb in sun.

		h	m
39°	.....	8	20
39·25	.....	8	34
39·5	.....	8	48
39·75	.....	8	58
39·1	.....	9	11
38·75	Greatest Phase	9	32
40·	.....	9	58
40·2	.....	10	3
40·5	.....	10	7
40·1	.....	10	19
42·75	.....	10	28
44·25	.....	10	40
43·4	End	10	50

Temperature in shade 4 feet 6 inches above ground.

Time of greatest obscuration	32°
At the end of eclipse	34·5

Temperature on ground.

Greatest obscuration	29°
End of eclipse	31·5

"On an Apparatus for determining the Horizontal Magnetic Intensity in Absolute Measure," by J. P. JOULE, LL.D., F.R.S., &c., V.P.

Many years ago Weber described a small portable apparatus for ascertaining the horizontal component of the intensity of the earth's magnetism. In it a magnetic bar, about four inches long, was vibrated under the influence of the earth's magnetism, and afterwards employed to deflect a compass needle. The moment of the deflecting bar was determined from its dimensions and weight, the compass needle was supported on a point in the ordinary manner, and no allowance was made for the influence of the earth in increasing the strength of the magnet while vibrating in the magnetic meridian. Notwithstanding these defects, the results obtained were of considerable accuracy, and justified the employment of the portable apparatus by travellers. I have endeavoured to improve Weber's apparatus, so as to give it an accuracy comparable to that of the instruments employed in stationary observatories, without increasing its expense or diminishing its portability. I will, with these preliminary observations, at once commence by describing the various parts of the apparatus I have constructed for my own use.

I employ two deflecting bars, for reasons which will afterwards appear. It is important that the length of the bars should be decreased as far as possible. By so doing, there

is greater probability of obtaining perfect homogeneity and accuracy of dimension, whilst they may be more readily and thoroughly magnetized. I have, after much trial and consideration, fixed upon two inches as the most convenient length, and on one quarter of an inch as the section of the bar. The bars are first filed, then thoroughly hardened, and afterwards ground to an exact figure, the operations being finished by rubbing the bars together with a little cutting powder between them. The bars may easily be made so true that when dry and perfectly polished, they will adhere strongly together. When finished, they must be measured by a standard rule under a microscope—the means of observations in many places being taken for the respective dimensions. Their weight must also be accurately determined.

The moment of a homogeneous parallelopiped in vibration is found by multiplying the square of the diagonal of the upper surface by the weight of the bar, and dividing by 12. If it be thought desirable to test the accuracy of the moment thus deduced, the best plan is to fasten small cubes of lead to the ends of the bar, by an elastic band. These cubes must be carefully wrought and measured, and their moment may be found by subtracting the moment of a bar of lead two inches long from that of a bar two inches long, plus the length of the two pieces. When the moment of the pieces of lead is determined, that of the steel bar may be found by vibrating it first by itself, and then with the lead cubes attached. Another plan is to hang pieces of lead from the ends of a filament of silk thrown over the bar, but in this case great care is requisite that the filaments have very little force of torsion, and that the vibration is very slow. The moments of the four bars I have finished, as determined by the above means, are as follows :—

	By Measurement.	By Fixed Lead Weights.	By Suspended Lead Weights.	Average.
No. 1	86·246	86·173	86·225	86·215
No. 2	86·424	86·330	86·435	86·396
No. 3	85·248	85·070	85·283	85·200
No. 4	84·843	84·773	84·906	84·841

The results show that the moments of steel bars, when carefully finished, may be very correctly deduced from their linear dimensions and weight.

It is important that the bars should be thoroughly and uniformly magnetized. This is best effected by bringing the ends of the bar into contact with the ends of two straight electro-magnets of considerable power. In this way the confused polarity, which more or less exists in all bars magnetized by stroking, is avoided—the result being, that the magnetic virtue is more permanent, and at the same time, much more effective.

The needle to be deflected should be of small dimensions, not exceeding half an inch in length. It may be conveniently made of several perfectly hard pieces of common sewing needle mounted on perforated card. A glass index should be affixed to it, the ends being furnished with very fine bits of copper wire. These show well when traversing the graduated circle.

A circle of six inches diameter will be found a convenient size. It should be divided to thirds of a degree. The deflections may be read off with sufficient accuracy by the aid of an eye-glass, a piece of looking-glass being placed on the table to ensure the correct perpendicularity of the eye. It is easy after a little practice, if read off the deflections to half a minute or less. The needle should be protected from currents of air by being placed with its circle in a shallow box covered by a glass plate, in the centre of which there is a hole for the purpose of suspension.

A filament of silk of six inches length, and strong enough

to carry the needle with safety, will be found to give a quantity of torsion so small that it may be neglected, not amounting to more than a small fraction of a minute in a deflection of 45°. For suspending the deflecting bars a stronger thread is required, but by increasing its length to four or or five feet the effect of torsion in this latter case also becomes too small to require notice.

The remaining part of the apparatus gives the means of placing the deflecting bars at accurately measured distances from the needle. This has generally been done by means of a divided metal rod; but I find that a plain board of well-seasoned deal, with a circular hollow excavation in the centre for the graduated circle, answers the purpose very well. It is needful to be able to place the bars rapidly to an exact position on the board. This is best done for each distance by driving pins into the board, and cutting them off pretty close to the wood, one pin regulating the distance and two others the direction. It is only necessary to push the bar up to its defined position. The distances of the pins on the east side of the suspended needle from the corresponding ones on the west side are in my own apparatus 15, 20, 25, and 30 inches, half those distances being the mean distances of the bar from the needle. The first two distances are, however, all that are actually required.

In making the observations of deflection two bars are used, one on the east side, the other on the west of the needle. They must be carried to their respective places by wooden pincers, and on no account touched by the fingers or subjected to any violence. The deflection marked by the pointer at both ends of the needle must be observed, and then the bars must be reversed and the deflection in the other direction also observed. The bars must then be removed to the next distances and the observations repeated. Finally the bars must be made to change sides from east to west, and all the former observations repeated in the reverse

order. Thus eight separate observations are obtained, of which the average gives the deflection due to any given distance.

The only further observation required for finding the earth's horizontal intensity is that of the number of vibrations of the bars under the influence of the earth's magnetism. It is clear that when a bar is vibrating about the magnetic meridian its magnetism is slightly increased by the inductive influence of the earth, and that thus the number of vibrations is increased. The use of two bars enables me to get rid of this difficulty in a very simple manner. I find the distance at which one bar must be held below the other so as to counteract the earth's magnetism; then I suspend the two bars, one beneath the other, at that interval, and vibrate both together. The result gives the number of vibrations due to the magnetism of the bars as uninfluenced by induction.

The method of suspension I have found most convenient is as follows:—A fine wire is twisted round the centre of one of the bars, forming a loop which is caught by a small hook fastened to the extremity of the suspending fibre. Then a filament of silk of the proper length, tied at the ends so as to form a long loop, is thrown along the length of the suspended bar, and the ends of the other bar are placed in the ends of the loop. There is some slight difficulty in making the bars hang quite horizontally, but this may be overcome by a little practice. The proof of accurate horizonal position is made by looking at the bar and a spirit level simultaneously from a distance with a telescope.

The bars being arranged as above in one system, and having nearly the same amount of magnetism in each, vibrate with perfect regularity. To ascertain the time occupied by the vibrations I look at the system through a telescope placed at a distance of two or three yards, a clock beating seconds being within hearing. Then I have a slate divided

into ten columns, and each time that I note the end of a vibration I make a mark on the slate, counting at the same time the ticking of the clock. Every time that the end of a vibration appears to be sensibly coincident with the beat of the clock I write the number of seconds on the slate alongside the usual mark. After observing the vibrations, which ought not to exceed 3° of amplitude, for about ten minutes, materials will have been collected for obtaining a sufficiently accurate determination of the time of vibration by dividing the times between the several coincidences by the number of intervening vibrations and taking the mean of the whole.

As has been shown by Gauss and Weber, if M be the force of the magnetic bars in absolute measure, θ the angle of deflection produced at the distance R, and T the absolute horizontal intensity of the earth's magnetism, $\frac{M}{T}=\frac{R^3 \tan \theta}{2}$, but approximately only, because the deflecting power of a bar decreases in a more rapid ratio than the inverse cube of the distance, on account of its own magnetic length. Hence the necessity of a correction which may be found by observing the deflections at two distances R and R, from which we obtain $L^2=\frac{\theta R^3-\theta_1 R_1^3}{2(\theta R-\theta_1 R_1)}$, where L signifies the virtual half length of the bar. Then we obtain the closely correct value $\frac{M}{T}=\frac{R^3 \tan \theta}{2}-L^2 R \tan \theta$. Calling this quantity r, as in Gauss and Weber's equation, we have finally $T=\frac{1}{t}\sqrt{\frac{12C}{r}}$, where t is the time of vibration in seconds, C is the moment of inertia of the bars multiplied by π^2, and 12 is the coefficient to reduce from inches to feet.

The following were the data for determining the horizontal intensity on four several occasions.

	Jan. 29th, 1867.	Feb. 5th, 1867.	Feb. 11th, 1867.	March 16th, 1867.
C	1691·80	1690·04	1690·04	1690·04
R	7·5	7·5	7·5	10
R_1	10	10	10	12·5
$\tan\theta$	·913087	·97852	·97520	·40610
$\tan\theta_1$	·379294	·40607	·40480	·20656
t	2·8407	2·7445	2·7483	2·7423
L^2	·9679	1·028	1·0122	·9007
r	185·976	198·861	198·303	199·395
T	3·6779	3·6795	3·6796	3·6776

The above determinations were obtained in a room in which there are several pieces of iron in fixed positions, and in the neighbourhood of which are some considerable masses of that metal. I have ascertained that these had an influence in increasing the intensity by about a fortieth. My current-meter, by which the horizontal intensity is immediately deduced from a comparison of its weighings with the deflections of a tangent galvanometer included in the same circuit, shows a gradual increase of the intensity since last September, conformably with the results obtained in past years at Kew.

"Note on the Tangent Galvanometer," by J. P. Joule, LL.D., F.R.S., &c., V.P.

It is well known that a current circulating round a magnetic needle produces a deflection of the latter, the tangent of which is approximately proportional to the strength of the current. Pouillet made the approximation much more close by using a broad strip of copper bent into a circle, instead of the usual coil, and thus obtained the instrument commonly known as the tangent galvanometer. The arrangement of Pouillet ensures a close approximation, it is true, but it does not, after all, exactly fulfil the law of

tangents; and in absolute measurements it has the disadvantage that the diameter of the circle of ribbon cannot be taken as the virtual diameter. It appeared to me that the best plan for a galvanometer would be to use a wire of about one-tenth of an inch diameter, surrounding a short needle, and to supply the small correction needed to the tangents of the deflections of the latter.

Calling, then, θ the angle of deflection, l the magnetical length of the needle (generally about four-fifths of the actual length), and d the diameter of the coil, we have

$$\{(2\sin\theta)^2 - (\cos\theta)^2\}\left(\frac{l}{d}\right)^2 \tan\theta,$$

or, more elegantly and simply, as suggested to me by Professor Jack,

$$\frac{1}{2}(4\tan^2\theta - 1)\frac{l^2}{d^2}\cdot\sin 2\theta,$$

for the correction to be supplied to the tangent of the angle observed. This correction is additive at great deflections, and subtractive at small ones, whilst at 26° 34′ the correction vanishes.

It seems, therefore, desirable that in exact researches a needle of small length should be employed, and the apparatus so arranged as to give a deflection somewhere about the above-indicated angle, where the correction, if needed at all, would be of trifling amount.

"On a Section of the Drift Deposits in the Banks of the Ribble, near Balderston Hall," by EDWARD HULL, B.A., F.G.S.

Since my paper on the "Drift Deposits in the neighbourhood of Manchester," &c., was published in the memoirs of the Literary and Philosophical Society of Manchester,* I have had opportunities of considerably extending my acquaint-

* Mem. Lit. & Phil. Soc., Manchester, vol. ii., 3 series, p. 449.

ance with these quaternary beds in a northerly direction, as far as the banks of the Ribble. What I have seen in these districts has fully borne out the statement which I made in that paper, that the sand and gravel which rests on the Lower Till, or Boulder Clay, is, in turn, itself "overlain by an Upper Till, or Boulder Clay, quite as important, both in thickness and extent as that which lies below it." The succession of these drift deposits which I then ventured to submit in modification of that by Mr. Binney, F.R.S.,* was as follows :—

Recent...	1.	Valley Gravel and River Terraces.
Drift...	2.	Upper Boulder Clay, or Till.
	3.	Middle Sand and Gravel.
	4.	Lower Boulder Clay, or Till.

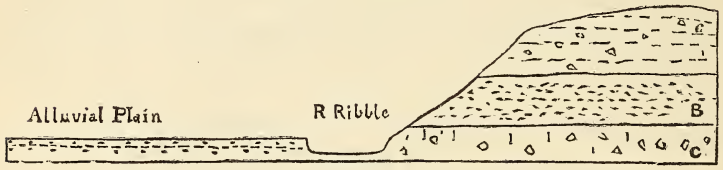
I wish now to describe a remarkable section visited by me a few months since on the Banks of the Ribble at Balderston, some distance below Ribchester. It is shown on the south bank of the river at Jackson's Bank Wood. In this district, the Ribble meanders over a wide alluvial plain, hollowed out of a larger valley of older date, which is overspread to a great depth by drift deposits, so that it is only at intervals the solid formations appear from below, even in the bed of the river itself. At the elbows of the river the banks, which are often 150 to 200 feet in height, are generally composed of quaternary deposits, and the river is ever wearing away these soft materials, which frequently come down in great landslips, and are carried downwards to the sea. My visit was made a few days after the great flood of last November, when the river rose 50 feet above its bed—overflowing the alluvial plain, and bringing down vast masses of its banks.

At Jackson's Bank Wood the river makes a fine sweep,

* "On the Drift Deposits of Manchester and its Neighbourhood." Mem. Lit. & Phil. Soc., vol. viii., 2 series.

and the section, which is 200 feet in depth, is very clearly laid open throughout a horizontal distance of several hundred yards. It is represented in cross section in the figure below.

Section of Drift Deposits in the Banks of the Ribble at Balderston.



c *Upper Till.* Reddish Till, from 70 to 80 feet in thickness.

B *Middle Sand.* Yellow and white soft sand, with beds of gravel, from 85 to 90 feet.

C *Lower Till.* Dark blue stiff clay, forming the bottom of the bank, about 25 to 30 feet in thickness.

The line of demarcation between the several members of the drift in this section is remarkably clear, and shows the great importance of the Upper Till, which forms the top of the bank for a depth of about 85 or 90 feet, and spreads over the country for miles. If there were no other section than this it would be sufficient to establish its place in the series of Drift Deposits.

In this section, and in some others I have noticed, there appears to be a difference between the Upper and Lower beds of Till. The Upper seemed to partake of a laminated or bedded structure, while the lower (as far as it was possible to judge at some distance, as it was inaccessible) seemed to want any appearance of stratification, and to correspond rather to the description which Mr. Geikie gives of the Lower Till of Scotland. This he considers to have been

formed by land ice, and in Scotland is generally destitute of any true stratification. In Lancashire, however, I have several times observed the Lower Till to be truly bedded; and I doubt much whether we have anything which exactly corresponds to the Lower Till of Scotland, at least until we approach the hills of the Borders and of the Lake districts.

At Blackburn the Upper Till descends into, and forms, the valley on which the town is built. It is in some places several yards in thickness resting on the Middle Sand, which at Pleasanton and Hoghton produces an undulating country, sometimes assuming the form of sand-dunes.

I shall conclude this notice with one remark. There are few things more deceptive than steep sections in drift deposits on the banks of a river or brook; and unless the observer is aware of the fact, he is liable to error. In cases where Till forms the upper part of the bank and rests upon beds of sand, the latter is sometimes completely hidden from view by a thick wash of the Till, which then seems to form the whole cliff. This is sometimes the case with the banks of the Ribble, and arises from two causes: The first is—that the sand is easily undermined by the stream, and giving way, the Red Till comes slipping down from above, and being more plastic, or cohesive, forms a covering for the whole surface. The second cause is—the rain, which softens the clay, and when a heavy fall happens, it is carried down in a semi-fluid state and spread over the underlying beds. It is, therefore, by no means common to find so fine a section, showing the three members of the drift series in their respective order and thickness, as that described at Balder-

ston, which is my apology for bringing it before the notice of the Society.

I may state that the height of the cliff is taken from the contour lines on the Ordnance 6-inch map, and the thickness of the different members, by estimating by the eye their relative proportions from a point on a level with the centre of the cliff.

PHOTOGRAPHICAL SECTION.

March 12th, 1867.

SAMUEL COTTAM, Esq., in the Chair.

Mr. WARDLEY said that he had observed recently in the Photographic Journals that complaints were made as to the appearance of streaks on collodion plates in the direction of the dip. He had never until lately seen anything of the kind, but he had been using some collodion which produced the markings, and he noticed that the streaks could be seen on the plates when taken out of the bath, and before exposure. The marks were not such as would be removed by re-dipping the plate and moving it from side to side in the solution; and he thought the cause would be found to arise from the collodion being over iodized. He had used collodion containing about one-eighth less iodizing solution and found that the streaks had disappeared.

Mr. BROTHERS, F.R.A.S., exhibited a number of photographs taken during the Eclipse of the Sun on the 6th inst. He said that he had anticipated some difficulty in photographing the sun from the excess of light causing over exposure, as the image was taken at the focus of the telescope without enlargement through an eyepiece. To overcome that difficulty the aperture of the telescope was reduced first to two inches, and then to half an inch; and collodion and developing solution which had been mixed upwards of twelve months, and an old silver bath were used. The exposure was as instantaneous as could be made by hand.

In explanation of the four prints being on one plate, Mr. Brothers stated that the slide he used for astronomical photography is arranged so that four negatives of the sun or moon can be taken on the same plate, an advantage which he had found of much importance in this kind of work. The time occupied in taking the four negatives on plate No. 3 was thirty seconds, No. 4 twenty-five seconds; and the last set was taken in *twenty seconds*—an average of *five seconds* for exposing and changing the position of the plate.

Two of the prints, 3 and 4 of the series, were mounted, in reversed order, for the stereoscope, and they exhibit very good stereoscopic effects although taken from the same station, the difference in the position of the moon in an interval of about seven minutes being sufficient for the purpose. The negatives on No. 3 were taken at $9^h\ 32^m$ to $9^h\ 32^m\ 30^s$, and those on No. 4 at $9^h\ 39^m$ to $9^h\ 39^m\ 25^s$

Mr. Brothers also exhibited a photograph on canvass printed without the aid of bitumen of Judea or gelatine, or of any other substance likely to change by time. The print is so completely stained into the canvass as prepared in the ordinary way for artists, that it cannot chip or rub off. Another advantage is that hyposulphite of soda is not required to fix the image.

Ordinary Meeting, April 2nd, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Sir Henry Holland, Bart., M.D., F.R.S., &c., was elected an Honorary Member of the Society.

Messrs. G. C. Lowe and W. L. Dickinson were appointed Auditors of the Treasurer's accounts.

"On the Microscopical Examination of Coal Ash or Dust from the Flue of a Furnace, illustrated by the Microscope," by J. B. DANCER, F.R.A.S.

When coal is burnt in a furnace to which atmospheric air has free access, a portion is converted into gaseous and volatile matter; and the incombustible substance which remains is the ash. The amount of ash in coals from different localities is very variable; it is said to range from 1 to 35 per cent. The ash or dust which is the subject of this paper was collected from the flue of my steam boiler furnace, in which common engine coal is used as fuel. This coal leaves a considerable amount of incombustible matter. A specimen of the dust is now before you; it is of a reddish brown colour, and free from soot or carbonaceous particles.* When this dust is examined under the microscope with a power of 40 or 50 diameters, it is found to consist of ferruginous matter and crystallised substances, some par-

* My attention was drawn to this subject by Mr. Johnson, of Wigan, in November, 1866.

ticles transparent, others white and red. It contains also a number of curious looking objects, which vary considerably in size and colour; the majority of these bodies are spherical, and when separated from the irregularly shaped particles forming the bulk of the dust they become interesting objects for the microscope. I shall confine my remarks more especially to these globular bodies. Some of these are as perfect in form as the most carefully turned billiard balls, and have a brilliant polish. The various colours which these globules exhibit give additional interest to their examination. Some are transparent crystal spheres, others are opaque white, many are yellow and brown, and variegated like polished agates or carnelian of different shades. The most abundant of the highly polished balls are black; there are others which look like rusty cannon balls—some of these have an aperture in them like a bomb shell, and many are perforated in all directions. To obtain these objects the dust should be washed in a bowl and all the lightest particles allowed to float away; the remainder consists of fragmentary crystalline and ferruginous substances; mixed with these are the polished balls described, which under the microscope, by a brilliant reflected light, look like little gems. To separate the spherical bodies from the irregular ones it is only necessary to sprinkle some of this material on an inclined glass plate, and by gentle vibration the balls roll down, and can thus be collected. Having satisfied ourselves with the examination under the microscope, it is natural that we should desire to know more about these novel objects. What is their elementary constitution? Why are they spherical? How do they get into the flue? I have not attempted a chemical analysis of these minute bodies, many of which are less than the 100th part of an inch in diameter. I can only therefore offer an opinion as to their probable constitution, judging from what is known of the chemical analysis of coal ash, and from the appearance

they present under the microscope. Referring to the chemical analysis of coal ash, we find that it sometimes contains silica, magnesia, alumina, sesqui-oxide of iron, lime, soda, potash, sulphate of calcium, anhydrous sulphuric acid, anhydrous phosphoric acid, sulphur, and sometimes traces of copper and lead. The vegetable origin of coal is now generally admitted, and doubtless some of the substances I have just named have been taken up by the coal plants, whilst other portions may have collected in the locality where the coal was formed. As this is not immediately connected with our present inquiry, I proceed to speculate as to the constitution of these globular bodies. The transparent spheres I imagine to be silicates of soda or potash; the opaque white are most likely silicate of soda or potash combined with lime and alumina; the yellow and brown are silicates coloured by iron in different proportions. The black globes are not all alike in composition; some of these are silicates coloured by carbon, others are iron balls coated externally with a silicate. Many of these rusty cannon balls are probably ferrous oxide formed by the action of heat on the iron pyrites in the coal. There are also balls of black magnetic oxide: the perforated shells are probably ferrous sulphides, The globular form of these bodies suggests that they have been thrown off in scintillations, such as are seen during the combustion of iron in oxygen gas, and whilst in a fluid state they assume a spheroidal form. They are carried by the draught into the flue, and being of greater specific gravity than the carbonaceous matter forming the smoke, they fall before the current of air has reached the chimney. Some of the dust has been a considerable time in the flue, exposed to the intensely heated circulating flame; the reducing action of this would probably convert some of the oxide into metallic iron. Many of these balls have the appearance of reduced oxides. The flue dust contains a larger amount of ferruginous

matter than can be accounted for by the analysis of coal ash. I think the surplus may be regarded as representing the wear and tear of the iron work about the furnace, such as fire bars, boiler plates, &c. The brick work and cement about the boiler and flues may also supply some of the silica, alumina, and iron for these balls, numbers of which are merely thin shells. The movements of these objects, caused by the approach of a magnet under the stage of the microscope, are somewhat amusing, and it is at times startling to see the crystalline objects, both spherical and irregular, exhibit magnetic attraction: probably they contain particles of iron imbedded in them; if they do not, may we not imagine that there is some magnetic compound in which the crystalline matter predominates? When we consider the accidental condition under which this matter has combined, it is just possible that some new molecular arrangement or combination of elements may have taken place. It is very probable that many of these polished balls are much more complex in their elementary constitution than I have stated. They are in fact a kind of glass, and many of them merely bulbs. Pelouze states that glass is probably an indefinite mixture of definite silicates. Glass, containing small quantities of ferrous oxides and sodic sulphates, when exposed to sunlight becomes yellow, and possibly some of these balls may have changed in colour since they came from the flue. Hydrochloric and nitric acid exert very little action on the ferruginous globes: this may be due in some measure to the high temperature at which the oxides have been formed; in other cases they are no doubt protected by an external coating of some silicate. It would require much time and patience to collect a sufficient number of each kind of these minute objects for a chemical analysis; but the spectroscope might probably assist in revealing their constitution. When time permits I hope to resume the subject.

"On the Galvanometer," by WILLIAM JACK, M.A., Professor of Natural Philosophy at Owens College.

A current of intensity i passes through a circular wire (radius r) fixed in a vertical plane passing through the magnetic meridian. A needle of length l (between the poles) is suspended in the usual way, on a fine point, situated on the axis of the plane of the coil, at a distance e from its centre, and can rotate round the point in a horizontal plane. Let E represent the earth's magnetic intensity, α the angle of deviation of the needle, and $\rho = \sqrt{e^2 + r^2}$, the distance of the needle point from the circumference of the coil. Then

$$\mathrm{E}\tan\alpha = \frac{2\pi i r^2}{\rho^3}\left(1 - 3\frac{l^2(4e^2 - r^2)(1 - 5\sin^2\alpha)}{4\rho^4} + \frac{45l^4}{64\rho^8}(8e^4 - 12e^2r^2 + r^4)\right.$$
$$\left.(1 - 14\sin^2\alpha + 21\sin^4\alpha)\right)$$

(Wiedemann, *Galvanismus*, vol. 2, p. 165.)

Accordingly,

$$i = \tan\alpha\frac{\rho^3\mathrm{E}}{2\pi r^2}\left\{1 - 3\frac{l^2(4e^2 - r^2)(1 - 5\sin^2\alpha)}{4\rho^4} + \frac{45l^4}{64\rho^8}(8e^4 - 12e^2r^2 + r^4)\right.$$
$$\left.(1 - 14\sin^2\alpha + 21\sin^4\alpha)\right\}^{-1} = \tan\alpha\frac{\rho^3\mathrm{E}}{2\pi r^2}\left\{1 + 3\frac{l^2(4e^2 - r^2)(1 - 5\sin^2\alpha)}{4\rho^4}\right.$$
$$- \frac{45l^4}{64\rho^8}(8e^4 - 12e^2r^2 + r^4)(1 - 14\sin^2\alpha + 21\sin^4\alpha)$$
$$\left. + \frac{36l^4}{64\rho^8}(16e^4 - 8e^2r^2 + r^4)(1 - 10\sin^2\alpha + 25\sin^4\alpha)\right\}$$
$$+ \text{ higher powers in } \frac{l^2}{\rho^4} \text{ which may be disregarded when } l \text{ is small compared with } \rho.$$

As a first approximation, therefore, we have

$$i = \frac{\rho^3\mathrm{E}}{2\pi r^2}\tan\alpha,$$

which shows that to this approximation the intensity varies as the tangent of deflection.

The first term to be applied in correction is

$$\frac{\rho^3\mathrm{E}}{2\pi r^2}\tan\alpha\frac{3l^2}{4\rho^4}(4e^2 - r^2)(1 - 5\sin^2\alpha) \quad \text{(A)}$$

This becomes 0 in two cases,

(1) $1—5\sin^2\alpha = 0$, $\therefore \sin^2\alpha = \frac{1}{5}$, $\tan^2\alpha = \frac{1}{4}$

so that α is the angle whose tangent is $\frac{1}{2}$.

This is the angle at which Dr. Joule has pointed out that it is best to work for accurate measurements. The first term (A) of the total correction will disappear, therefore, even though the centre of the magnetic needle may not be accurately in the plane of the coil, provided it is on a line perpendicular to the plane passing through the centre. When we work at any other angle the correction is

$$\frac{3l^2\mathrm{E}}{8\pi r^2}\tan\alpha\frac{(4e^2—r^2)}{(e^2+r^2)^{\frac{1}{2}}}(1—5\sin^2\alpha)$$

The magnitude of the correction will depend therefore on that of the term

$$\frac{(4e^2—r^2)}{(e^2+r^2)^{\frac{1}{2}}}\tan\alpha,$$

and will still become zero whatever be the value of $1—5\sin^2\alpha$, provided $4e^2—r^2=0$.

The same accuracy is therefore secured by working at any angle with a needle whose centre is at a distance equal to half the radius of the coil from its centre, and along a line perpendicular to its plane.

The next term of the correction becomes

$$\frac{\rho^3\mathrm{E}}{2\pi r^2}\tan\alpha\frac{27l^4}{40\rho^8}(8e^4—12e^2r^2+r^4)$$

for the angle α specified. For $e=0$ (needle at centre), this gives

$$\left(\rho=r\right),\quad \frac{r^3\mathrm{E}\tan\alpha}{2\pi r^2}\cdot\frac{27l^4}{40r^8}\cdot r^4 = \mathrm{E}\tan\alpha.\frac{27l^4}{80\pi r^3}$$

For $e=\frac{r}{2}$, it gives $l^2=e^2+r^2=\frac{5r^2}{4}$

$$\mathrm{E}.\tan\alpha\cdot\frac{27}{80\pi}\cdot l^4\cdot\frac{\rho^3}{\rho^8}\times—\frac{3r^2}{2} = —\mathrm{E}.\tan\alpha.\frac{27}{80\pi}\frac{l^4}{r^3}\frac{\frac{3}{2}}{\frac{25\sqrt{5}}{2^5}}$$

$$= —\mathrm{E}.\tan\alpha.\frac{27}{80\pi}\cdot\frac{l^4}{r^3}\frac{48}{25\sqrt{5}}$$

The last correction is therefore rather less than the former. There is, therefore, a slight positive advantage in working with the needle centered at $e = \frac{r}{2}$, over Dr. Joule's method, even at the special angle $\alpha = \tan^{-1}\frac{1}{2}$, and in addition the first term of the correction disappears for *all* angles of deviation, instead of for one only.

The accuracy of the formula quoted above turns on the circuit being complete (as it never is), on its being accurately circular, in the vertical meridian plane, and on the r being a definite quantity. If the thickness of the coil be comparable to the magnitudes r and l used, a much more complicated formula would replace that given above.

The instrument in which the needle pivot is at $e = \frac{r}{2}$, possesses this advantage, that we may arrange a considerable number of coils for which this is true, parallel to each other, along the smooth surface of a cone whose axis is $\frac{r}{2}$, and whose base has the radius r. It is to be observed, however, that though the second term in the value for i given by each separate parallel coil disappears, the first term

$$\tan\alpha \frac{E}{2\pi}\frac{\rho^3}{r^2} \text{ becomes } \tan\alpha \frac{E}{2\pi}\frac{5\sqrt{5}}{8}r$$

and therefore for one same current circulating through several coils, for each of which there is a different radius, there will not be accurately one same amount of deviation produced by each. Accordingly, the current through two coils has not accurately the effect of a current of double intensity, and has only that effect approximately, so long as the r of the parallel coils is very nearly the same. In fact, no multiplying instruments can approach the accuracy of the ordinary circular single meridian coil with the needle in the centre, when it is worked at $\alpha = \tan^{-1}\frac{1}{2}$, (Joule's arrangement), or of the circular single meridian

coil with the needle in axis of coil at distance $e = \frac{r}{2}$ from centre (Gaugains' arrangement).

A curious result of the formula (A) given for the first correction is that this correction is a maximum

$$\text{for} \quad 1 - 4\tan^2\alpha - 10\sin^2\alpha = 0$$

$$\text{or} \quad \sin^2\alpha = \frac{3 - \sqrt{7.4}}{4}$$

Which corresponds to an angle 15° 20′. At this angle, therefore, the correction that must be applied is greatest for all tangent galvanometers. Hence, in delicate research where the needle is in the centre of the instrument, and when accuracy is of great importance, we should take care that our deviations shall not be at or near this amount. In Gaugains' arrangement this angle is of no special importance.

A simple numerical calculation may show the comparative insignificance of the correction remaining when either arrangement is adopted. If we call I the first approximate value of i

$$i = I\left(1 - \frac{45l^4}{64\rho^8}(8\ e^4 - 12\ e^2r^2 + r^4)\ (1 - 14\ \sin^2\alpha + 21\ \sin^4\alpha\right)$$

Take $l = \frac{1}{2}$ inch, $r = 10$ inches. Then

(1) Joule's arrangement, $\alpha = \tan^{-1}\frac{1}{2}$, $E = 0$

$$i = I\left(1 - \frac{45.}{64} \cdot \left(\frac{1}{20}\right)^4 \times \frac{24}{25}\right)$$

$$= I(1 + \cdot 000004)$$

(2) Gaugains' arrangement,

$$\alpha \text{ any value } e = \frac{r}{2}$$

$$i = I(1 + \cdot 00000027)(1 - 14\ \sin^2\alpha + 21\ \sin^4\alpha)$$

a fraction depending on α, but always excessively small. It is obvious that inevitable instrumental errors must always produce corrections more considerable than either of these.

"Additional Note on the Galvanometer," by Dr. J. P. Joule, F.R.S., V.P.

The current-meter has enabled me to find experimentally the diminution of deflection consequent on an increase of the length of the needle. This was done by comparing the square roots of the weighings of the meter with the deflections occasioned by a coil 15·2 inches diameter in the centre of which needles of 1, 2, and 4 inches length were successively placed. I find the fractions representing the loss on the tangents of deflection as follow:

	From 1in. to 2in. Needle.	From 1in. to 4in. Needle.
68° 21′	·02239 loss	·11258 loss
42°	·00993 loss	·04870 loss
27° 2′	·00023 gain	·00254 loss
19° 1′	·00287 gain	·01646 gain

Hence the total fractions gained or lost by the two needles are:

	For 2in. Needle.	For 4in. Needle.
68° 21′	·02985 loss	·12009 loss
42°	·01324 loss	·05194 loss
27° 2′	·00032 gain	·00230 loss
19° 1′	·00383 gain	·01756 gain

Since my last communication I have made trial of many bars and needles, and find that the magnetic length is about $\frac{7}{8}$th of the actual in most cases. If therefore instead of l we take the actual length of the needle n, the correction to be applied to the tangent of deflection will be nearly

$$\frac{1}{4}\left(4\tan^2\theta - 1\right)\frac{n^2}{d^2}\sin 2\theta.$$

The fractions of loss or gain calculated from this formula for the deflections experimented with are as follow:

	For 2in. Needle.	For 4in. Needle.
68° 21′	·02873 loss	·11493 loss
42°	·01072 loss	·04290 loss
27° 2′	·00029 loss	·00116 loss
19° 1′	·00406 gain	·01625 gain

Very curious phenomena present themselves as the needle is prolonged. There ultimately becomes a point of stable equilibrium on the other side of the meridian. The following rough results obtained with a coil of $7\frac{1}{4}$ inches diameter will give an idea of these changes:

Length of Needle.	Deflection on the usual side.	Deflection on the opposite side.	Unstable Equilibrium on the opposite side.
1in.	74°	—	—
2in.	72°	—	—
3in.	68°	—	—
$3\frac{1}{2}$in.	$65\frac{1}{2}$°	—	—
4in.	62°	—	—
$4\frac{1}{2}$in.	56°	—	—
5in.	50°	—	—
$5\frac{1}{2}$in.	$41\frac{1}{2}$°	—	—
6in.	34°	—	—
$6\frac{1}{2}$in.	23°	35°	35°
$6\frac{3}{4}$in.	21°	47°	26°
7in.	17°	57°	14°
$7\frac{1}{4}$in.	13°	59°	13°
$7\frac{1}{2}$in.	$8\frac{1}{2}$°	59°	6°
8in.	—	63°	
9in.	—	62°	
10in.	—	61°	
11in.	—	59°	
12in.	11°	58°	
13in.	17°	56°	
14in.	20°		
15in.	23°		

"Notes on the Smallest and Simplest Forms of Matter and its Relations to Mechanical Forces," by J. C. Dyer, Esq., V.P.

The author's object in this paper is not so much to enlarge the present sphere of our knowledge, as it is to show how little we really know, about the essence of

matter, or of the exertion of *any forces* by it, excepting such as are open to direct experiment and observation. If there be any recondite forces that sustain the variable forms of bodies, all we can know about them must be derived from the sensible properties of matter; for any assumed properties, or motions of bodies, or their component parts, if hidden from our powers of perception, they are so because they afford no tests, by weight or measure, of their existence as physical realities. Still, with a view to obtain some conception of the laws that govern the visible and tangible forms of bodies, several hypotheses have been suggested to account for phenomena attending the mutations of bodies. It has been *assumed* that certain *internal motions* and *forces* are called into action by the changes that take place in the visible forms of bodies—to explain these we have had the *theories* of *atoms*, of *corpuscules*, of *molecules* and the like. By these theoretic views of the composition of bodies, the smallest particles in them are supposed to be endowed with *functional properties*, which exert mechanical forces, according to the distances apart of the atoms and molecules, in all ponderable bodies. The author does not object to these theories, if they may in anywise serve to clear the paths of inquiry into the physical properties of bodies and their mutations; but whatever may be the *real* or *ideal* internal action of the particles of matter upon each other, or upon their masses, the questions they involve do not admit of any solution, or even elucidation, by any course of mathematical investigation that has been or can be applied to them, and this for the obvious reason that those particles (whether called atoms, molecules or otherwise) can have no conceivable weight or extension apart from their masses, and therefore no *functional value* can be assigned to them as the basis of any kind of moveable or statical forces in them. Whether such atoms and molecules be considered as in a state of

"vibration," "oscillation," "rotation," or as fixed by their balanced forces of attraction and repulsion, they are in each case absolutely beyond the grasp of the *mathematical* investigator, however profound he may be, because he has not the shadow of any ground for assigning either *size*, *motion*, or *force* to the minute parts of matter apart from its aggregate forms, and all we can even know on these obscure matters must be inferred from observed physical effects.

For reasons which he states the Author dissents from the dynamical theory of heat, and ventures to maintain the theory of a *calorific element* having not only the properties *sui generis* ascribed to it by the old school — Boerhaave to Dalton — but also as being the real source of all other forms of heat, and likewise as constituting in itself the essence of the elastic force in bodies.

"Variable Star Ephemeris for 1867, calculated from the Elements given in Dr. Schönfeld's Catalogue," by J. R. Hind, F.R.S., &c., Superintendent of the Nautical Almanac, and Honorary Member of the Society.

The times are those of *Maxima* unless otherwise stated.

R Androm. .....January 28.
T Piscium .....*Max.* April 4·4, and August 28·8.
Min. June 25·4, and November 18·8.
S Piscium .....February 12·4.
R Piscium .....1866, Dec. 17, and 1867, Nov. 29.
R Arietis .....February 19·3, and August 23·5.
o Ceti .....November 25·9.
R Persei .....June 29.
R Tauri .....September 15·7.
S Tauri .....May 21.
R Orionis.....August 22.
R Aurigæ.....December 6.
R Geminorum.....February 16·5.

R Canis Min. .....July 23.
S Canis Min. .....January 21, and December 23.
S Geminorum.....*Min.* June 8.
T Geminorum.....January 28, and November 11.
U Geminorum.....Feb. 11·0, May 19·1, Aug. 24·2, Nov. 29·3.
R Cancri .....October 15·6.
U Cancri .....July 13.
S Hydræ.....1866, December 21·4. and 1867, Sep. 3·8.
T Cancri .....*Min.* December 16.
T Hydræ.....August 24.
R Leonis Minoris ....March 13.
R Leonis .....September 24·6.
R Ursæ Maj. .....April 26.
S Leonis.....March 6, and September 12.
R Comæ .....August 30.
T Virginis .....October 16.
T Ursæ Maj. .....March 5·9, and Nov. 18·2.
R Virginis .....March 9.
S Ursæ Maj. .....March 10·6, and October 21·4.
U Virginis .....January 19·4, and August 15·0.
V Virginis .....January 13, and September 22.
R Hydræ.....December 17·0.
S Virginis .....June 13·6.
S Bootis .....January 3, and October 1.
R Camelop. .....June 20·8.
R Bootis .....July 28·5.
S Serpentis.....January 23.
S Coronæ .....June 29.
R Serpentis.....April 29.
R Libræ .....1868, March 27.
R Herculis .....1867, March 31.
U Herculis .....August 4.
S Ophiuchi .....February 9 and October 1.
S Herculis .....August 21.
R Ophiuchi .....July 30.
T Herculis .....*Max.* April 20, October 2.
Min. January 30, July 14, December 26.

R Lyræ .....Feb. 13, Mar. 31, May 16, Jy. 1, Aug. 16, Oc. 1. Nov. 16, 1868, Jan. 1.

R Aquilæ.....February 14.

R Sagittarii.....June 29.

R Cygni .....August 13.

S Vulpeculæ .....*Max.* Feb. 25·8, May 4·7, July 11·6, Sept. 17·6, Nov. 24·5.
Min. Jan. 26·0, April 3·9, June 10·8, Aug. 17·8, Oct. 24·7, Dec. 31·6.

χ Cygni .....,1866, Dec. 25·4.—1868, Jan. 15·5. No *max.* in 1867.

S Cygni .....April 15.

R Capricorni .....August 19.

S Aquilæ.....March 5, July 5·5, November 5.

R Sagittæ .....*Min.* March 4·2, May 13·8, July 23·4, Oct. 2·0, Dec. 11·5.

S Delphini .....July 22.

T Delphini .....May 31.

U Capricorni .....1868, January 9.

T Aquarii .....June 27.

R Vulpeculæ.....*Max.* April 20·0, and September 5·6.
Min. Feb. 18·0, July 7·0, and Nov. 22·0.

T Capricorni .....August 23.

S Cephei.....*Min.* March 27 ±.

μ Cephei.....*Min.* May 1, *Max.* 1868, January 12.

T Pegasi .....November 27.

S Aquarii .....August 2·5.

R Pegasi .....June 28.

R Aquarii .....1866, December 24, 1868, January 16.

R Cassiopiæ.....July 3·5.

Ordinary Meeting, April 16th, 1867.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Henry Charles Beasley was elected an Ordinary Member of the Society.

"On a New Form of the Dynamic Method for Measuring the Magnetic Dip," by Sir William Thomson, M.A., D.C.L., F.R.S., &c., Honorary Member of the Society.

Seven years ago an apparatus was constructed for the natural philosophy class of the University of Glasgow, for illustrating the induction of electric currents by the motion of a conductor across the lines of terrestrial magnetic force. This instrument consisted of a large circular coil of many turns of fine copper wire, made to rotate by wheel work about an axis, which can be set to positions inclined at all angles to the vertical. A fixed circle parallel to the plane containing these positions, measured the angles between them. The ends of the coil were connected with fixed electrodes, so adjusted as to reverse the connections every time the plane of the coil passes through the position perpendicular to that plane. When in use, the instrument should be set as nearly as may be in the magnetic meridian. The fixed electrodes being joined to the two ends of a coil

of a delicate Galvanometer, a large deflection is observed when the axis of rotation forms any considerable angle with the line of magnetic dip. On first trying the instrument I perceived that its sensibility was such as to promise an extremely sensitive means for measuring the dip. Accordingly, soon after I had a small and more portable instrument constructed for this special purpose; but up to this time I had not given it any sufficient trial. On the occasion of a recent visit, Dr. Joule assisted at some experiments with this instrument. The results have convinced us both that it will be quite practicable to improve it so that it may serve for a determination of the dip within a minute of angle. I hope, accordingly, before long to be able to communicate some decisive results to the society, and to describe a convenient instrument which may be practically useful for the observation of this element.

"Further Remarks on the Galvanometer," by WILLIAM JACK, M.A., Professor of Natural Philosophy at Owens College.

Dr. Joule's results, given in the last number of the *Proceedings*, suggested to me to inquire whether the ordinary formula which I gave in my last communication allowed a deviation of different sign for a short magnet and for one of considerable length. When the needle is at the centre of the circle, the formula gives, putting

$$\frac{l^2}{r^2} = n, \text{ and } \sin^2\alpha = x,$$

and making some simple transformations

$$\frac{64}{945}\cdot\frac{\mathrm{E}\cdot r\cdot\tan\alpha}{2\pi i} = \left(nx - \frac{315n+120}{945}\right)^2 - \frac{180.315}{945^2}\cdot\left\{n+\frac{84}{315}\right\}^2 - \frac{87696}{315^2}\Bigg\}$$
$$= \mathrm{E} \quad -\mathrm{F}$$

This always gives a + result for n small, as may be readily seen from the formula in its original shape. Its complete solution in x for every value of $n + i$ would require us to solve an algebraical equation of the form

$$A\sqrt{\frac{x}{1-x}} = (Bx-C)^2-D$$

which would be difficult, if possible. But we may see that the second term can never become negative, unless F is positive, since E is essentially positive. No negative result can be obtained, therefore, for tan α, unless

$$\left(n+\frac{84}{315}\right)^2 > \frac{87696}{315^2}$$

$$\therefore\ n > \frac{212\cdot1}{315}$$

Whenever n is greater than this value, E—F *may* become negative, and if it become so for some value of x ($=\sin^2\alpha$) less than 1, we may always adjust i so as to give $\frac{64}{945}\cdot\frac{E\cdot r\cdot\tan\alpha}{2\pi i}$ an equal value, where α is the negative angle in the first quadrant the square of whose sine is defined as above. Apparently, therefore, there ought to be at all events one solution implying deviation opposite to that for a small magnet, for some value of i, whenever $\frac{l^2}{r^2} > \frac{212\cdot1}{315}$.

The results in several cases in which different values have been assigned to $\frac{l^2}{r^2}$ are given below.

1. $\frac{l^2}{r^2} = \frac{212\cdot1}{315}$(limit value)

then E = 0, for $x = \frac{332\cdot1}{636\cdot3} = \sin^2\alpha$.

but in order that the original equation should hold, we must have i indefinitely great.

2. $\frac{l^2}{r^2} = \frac{216}{315} = \frac{24}{35}$,

A negative tan a, when $(648x - 336)^2 \angle 180 \cdot 315 \left(\frac{300^2}{315^2} \frac{87696}{315^2}\right)$

$$\text{for } x = \sin^2 a \angle \frac{372 \cdot 2}{648} \qquad a = a_1$$

$$\text{And also for } x \rangle \frac{300}{648} \qquad a = a_2$$

i being taken so as to give a deviation between a_1 and a_2, we shall have a possible solution in opposite direction to that for small magnet.

$$3. \quad \frac{l^2}{r^2} = 1, \quad x = \sin^2 a \angle \frac{637}{945} \rangle \frac{233}{945}.$$

These examples only profess to show that when the intensity is suitably taken, the result specified by Dr. Joule is verified. I should expect to find, when the intensity is gradually changed through a wide range, the sign of the deviation also change, and then, after a certain continued increase of i (longer continued, as l bears a greater proportion to n) generally change back again.

It will be noticed that $\frac{l^2}{r^2}$ must be $\rangle \frac{212 \cdot 1}{315}$.

Now Dr. Joule points out that if L = actual length of needle, $l = \frac{7}{8}$ L approximately. Assuming this

$$\therefore \frac{49}{64} \cdot \frac{L^2}{r^2} \rangle \frac{212 \cdot 1}{315}$$

$$\therefore \frac{L^2}{r^2} \rangle \frac{64}{49} \cdot \frac{212 \cdot 1}{315}$$

$$\therefore \frac{L}{r} \rangle \frac{8}{7} \cdot \times \cdot 82$$

$$\rangle 94$$

And $7\frac{1}{4}$ inches $\times \cdot 94 = 6 \cdot 8$ inches.

Dr. Joule finds the effect begins at 6·5 inches.

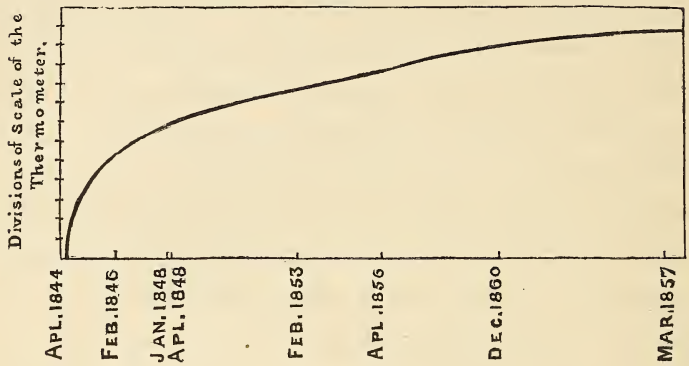
"Observations on the Alteration of the Freezing Point in Thermometers," by Dr. J. P. Joule, F.R.S., V.P.

Having had in my possession, and in frequent use, for nearly a quarter of a century, two thermometers, of which I have from time to time taken the freezing points, I think the results may offer some interest to the Society. Both thermometers are graduated on the stem, and are, I believe, the first in this country which were accurately calibrated. Thirteen divisions of one of them correspond to one degree Fahrenheit. It was made by Mr. Dancer, in the winter of 1843-44. My first observation of its freezing point was made in April, 1844. Calling this zero, my successive observations have given

0	April, 1844.
5·5	February, 1846.
6·6	January, 1848.
6·9	April, 1848.
8·8	February, 1853.
9·5	April, 1856.
11·1	December, 1860.
11·8	March, 1867.

The total rise has been, therefore, ·91 of a degree Fahrenheit. The other thermometer is not so sensitive, having less than four divisions to the degree. The total rise of its freezing point has been only ·6 of a degree; but this is probably owing to the time which elapsed between its construction and the first observation being rather greater than in the case of the other thermometer. The rise of the two thermometers has been almost identical during the last nineteen years.

A projection of the observations given above is shown in the following diagram :—



Mr. E. Bowman, M.A., and Mr. A. Brothers, F.R.A.S., illustrated the phenomena of "sonorous" and "sensitive" flames by a series of interesting experiments.

"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes, Part IV.," by James Nasmyth, Esq., C.E., Corresponding Member of the Society.

In this part of his paper the author gives detailed descriptions, illustrated by diagrams, of his methods of mounting the specula of reflecting telescopes, and of testing the figure of the great speculum; he also offers some very useful remarks on the general management of Newtonian reflecting telescopes, and on the atmospherical circumstances which affect their performance.

PHOTOGRAPHICAL SECTION.

April 9th, 1867.

J. Baxendell, F.R.A.S., Vice-President of the Section, in the Chair.

Mr. Brothers read the following "Note on Photography in 1787."

It is generally supposed that the earlier attempts to use nitrate of silver for producing pictures of lace, leaves, and other objects on white leather or paper were made by Wedgewood and Davy about the year 1802; but it will appear from the following extract that at least fifteen years earlier than the date named. and within ten years of the time when Scheele investigated the subject of the action of light on the salts of silver, the possibility of utilizing the action of light was known. The title of the book from which the extract is taken is "Rational Recreations in Natural Philosophy," &c., by W. Hooper, M.D., 1787; and the paragraph is headed "How to print letters by sunlight."

"Dissolve chalk in aqua fortis to the consistence of milk, and add to that a strong dissolution of silver. Keep this liquor in a glass decanter well stopped, then cut out from a paper the letters you would have appear and paste the paper on the decanter, which you are to place in the sun in such a manner that its rays may pass through the places cut out of the paper and fall on the surface of the liquor. The part of the glass through which the rays pass will turn black, while that under the paper will remain white. You

must observe not to move the bottle during the time of the operation."

Mr. Coote exhibited some snow scenes, the negatives of which were taken on collodio-albumen plates. Some of these beautiful views were slightly defective in the high lights, a number of vein-like markings appearing in the sky and foreground.

Mr. Wardley stated that these defects were entirely caused in the development, and had no connection with the character of the collodion used, or with the preparation of the plate. He considered that the imperfections were produced entirely by the repellent or nonmiscible nature of the solutions, containing acids and salts, used in development, acetic acid being one of the chief causes of the defects. Another source of the evil may be a low temperature and the developing solution being allowed to rest, even for a moment, on the plate. Such defects may be produced in abundance on any kind of dry plate if the developing solution is allowed to rest.

Annual Meeting, April 30th, 1867.

Edward Schunck, Ph.D., F.R.S., F.C.S., &c., President, in the Chair.

Professor Joseph Henry, Secretary of the Smithsonian Institution, Washington, was elected an Honorary Member of the Society.

It was moved by Mr. Brothers, seconded by Mr. W. B. Johnson, and resolved unanimously,—"That the system of electing Sectional Associates be continued during the ensuing session."

The following Report of the Council was then read by one of the Secretaries :—

The Council again refer with satisfaction to the Treasurer's account, copies of which are already in the hands of the members. It will be seen that the balance in hand on the 1st of April, 1867, was £322 12s. 1d., against £269 4s. 3d. on the 1st of April, 1866.

The number of ordinary members on the roll of the Society on the 1st of April, 1866, was 183, and five new members have been elected during the year. The losses have been by resignations, four; defaulters, three; and one ordinary member elected an honorary member. The number of members on the roll on the 1st of April, 1867, was therefore 180.

It is a noteworthy circumstance that no deaths have occurred amongst the ordinary members during the year; but the Society has lost three of its honorary members—Professor Eilert Mitscherlich; Mr. John Mercer, F.R.S.; and William Hopkins, Esq., M.A., LL.D., F.R.S.; and two

corresponding members, Mr. Benjamin Dockray and Professor A. D. Bache. One new honorary and two corresponding members have been elected during the year.

Mr. Mercer was a remarkable instance of a self-made man. Beginning the world without any advantages derived from education or easy circumstances, by indomitable energy and perseverance he attained a considerable position both as a man of science and a manufacturer. The best portion of his life was devoted to calico printing, to the advancement of which he contributed by the invention of many useful and ingenious processes. He was born February 21st, 1791, at Great Harwood, near Accrington. His father had a small cotton factory, and he himself worked in early youth at the loom. In 1807, being then 16 years old, he was much struck by the bright colour of a child's dress, which was an orange dyed with annotto. The wish to dye this and similar colours possessed him, but he did not know how to begin. In an outline of his life, drawn up by Mr. Mercer for his sons, he says:—"How to begin was my difficulty. There was no fancy dyer in our village; I had no acquaintance with any one who knew anything of dyeing, and I had no books that would help me. The villagers wore mostly dyed cotton at that time, and got them dyed at Blackburn. I therefore went to a druggist at Blackburn, and got a little of all the kinds of stuffs the dyers used, *with the names written on each.* Weavers at this time having plenty of cotton fents, I was thus well provided with all that was required, and by ringing changes and labelling all the results I was able to dye all the colours then in use. Not having convenience where I lived, I took a man to join me, who had a sufficient building for a dyehouse, with water near, and we dyed for the inhabitants of Great Harwood and the surrounding villages." From 1809–10 Mr. Mercer was employed in the works of Mr. Fort, calico printer, at Oakenshaw, near Harwood. During the following years he devoted himself

with much ardour to the study of chemistry, to which his attention had been directed by the purchase at an old bookstall of a copy of "Parkinson's Pocket Chemistry." He then began to experiment on colours for calico printing, and at the end of 1817 had succeeded in applying some totally new colours to calico. Amongst these was the orange sulphuret of antimony, a colour so much brighter than the one up to that time dyed with annotto as to immediately attract the notice of the calico printers in the neighbourhood. His success led to his appointment as chemist in Mr. Fort's works, at Oakenshaw. In 1825 he was admitted a partner in the concern, and continued in this capacity till 1848, when the firm, having been very successful, gave up the business, Mr. Mercer retiring with a handsome fortune. He thenceforward devoted himself to theoretical chemistry, and was during later years in correspondence with many of the first chemists of the day.

It would be difficult to name all Mr. Mercer's inventions in connection with calico printing. It may suffice to mention those with which his name is chiefly associated. The long popular style of manganese brown, with white discharge and wood-colour illuminations, was his invention, and hundreds of thousands of pieces in this style must have been printed. He contributed largely to the chemistry of indigo dyeing, and invented an ingenious process for discharging indigo blue by means of alkali and red prussiate of potash. He was the original maker of dry stannate of soda, now so largely used by calico printers, Mr. Steiner having previously introduced the liquid stannate of potash, a much more costly article. He was the first to apply arseniates as a substitute for cow's dung in madder dyeing, phosphates having previously been employed for the same purpose. The chlorinating of woollen fabrics and mixed fabrics of wool and cotton, whereby a property of seizing and retaining colours with greater facility is given to wool, was, as all

printers of these fabrics will admit, a step of very great importance, and had Mr. Mercer patented this invention a very large sum would probably have accrued to him. It was, however, given to the trade with no reservation whatever. To Mr. Mercer must be attributed the discovery of the peculiar action of caustic soda and sulphuric acid upon cotton. This singular process, now called "mercerizing" has the effect of untwisting the normally twisted flattened tubes of cotton filaments and converting them into cylindrical tubes. When colours are applied to the cotton so treated they pass more readily through the minute pores of the tubes and are precipitated in denser layers in the interior of the latter, whereby darker and more permanent shades are produced. Calico so treated has become greatly increased in strength, and though hitherto no large quantities of cloth thus prepared have been printed, owing to the expense of preparation, advantage has been taken of the process to prepare the cotton fabric used in the production af the endless web known to calico printers as the indiarubber blanket, which, when made with prepared calico, is rendered much more durable,

Mr. Mercer acted as a chemical juror for the Great Exhibition of 1851, as well as that of 1862. In 1852 he was elected a Fellow of the Royal Society. He was an honorary member of this Society as well as of the Glasgow Philosophical Society. He was put on the commission of the peace in 1861. Mr. Mercer closed a long period of useful labour on November 30th, 1866, after several years of suffering, borne by him with great fortitude and resignation. He had endeared himself to all who knew him by his simplicity of character, large-hearted kindness, and purity of life. To the poor his hand was always freely extended, and having himself been one of the people, he knew how to sympathise with their sorrows and give aid where most wanted. Though dying at an advanced age, his loss is felt

by a large circle of friends as that of one whose powers of mind and virtues of heart were of no common order.

The following list of Papers and Communications read at the Ordinary and Sectional Meetings of the Society during the session now ending will show that there is no abatement in the scientific zeal and activity of the members and associates who contribute to the Society's Memoirs and Proceedings :—

October 2nd, 1866.—"On the Portraits of Sir Isaac Newton ; and particularly on one of him by Kneller, painted about the time of the publication of the *Principia*, and representing him as he was in the prime of life," by Samuel Crompton, M.D.

October 4th, 1866.—"On a Process for Printing Photographs in various Colours," by Mr. J. A. Gatty.

October 8th, 1866.—"On the Plants Springing up Spontaneously on the fresh turning up of the Pasture Land at Knutsford, Cheshire," by Mr. H. A. Hurst.

October 11th, 1866.—"Observations of the Eclipse of the Sun, October 8th, 1866, at Mr. Worthington's Observatory, Crumpsall," by Joseph Baxendell, F.R.A.S.

October 11th, 1866.—"Results of a Comparison of the Magnitudes of the Bedford Catalogue with those of Sir John Herschel," by George Knott, F.R.A.S.

October 11th, 1866.—"Note on the Combined Magnitude of Two Stars in Close Proximity," by George Knott, F.R.A.S.

October 11th, 1866.—Catalogue of Binary Stars, with Introductory Remarks," by Alfred Brothers, F.R.A.S.

October 16th, 1866.—"On the Isomorphism of Thallium-perchlorate with the Potassium and Ammonium-perchlorates," by Professor H. E. Roscoe, F.R.S., &c.

October 30th, 1866.—"On the Manufacture of Sulphide of Ammonium," by Peter Spence, F.S.A., &c.

November 8th, 1866.—"Observations of the New Variable Star T Coronæ," by Joseph Baxendell, F.R.A.S.

November 12th, 1866.—"On Mosses New to Great Britain," by Mr. E. G. Hunt.

November 13*th*, 1866.—"On Echinus Lividus, Illustrated by Specimens from Roundstone," by Thomas Alcock, M.D.

November 13*th*, 1866.—"On the Structure of the Spines of Echini," by Mr. H. A. Hurst.

November 27*th*, 1866.—"Observations of the Meteoric Shower of November 13-14, 1866," by Joseph Baxendell, F.R.A.S.

December 6*th*, 1866.—"Observation of the Occultation of Aldebaran, November 22nd, 1866," by Alfred Brothers, F.R.A.S.

December 6*th*, 1866.—"On some Recent Results of Observations of the Specific Gravity of Sea Water," by Thomas Heelis, F.R.A.S.

December 11*th*, 1866.—"On the Recent Suspension by the Board of Trade of Cautionary Storm Warnings," by Joseph Baxendell, F.R.A.S.

December 11*th*, 1866.—"On two Fossil Specimens of a Species of Calamites from the Carboniferous Strata of Great Britain," by E. W. Binney, F.R.S., F.G.S.

December 11*th*, 1866.—"On the Occultation of Aldebaran by the Moon, January 16, and on the Eclipse of the Sun, March 5, 1867," by Mr. W. L. Dickinson.

December 11*th*, 1866.—"Notes on Varieties of Sarothamnus Scoparius and Stachys Betonica, from the Lizard, Cornwall," by Mr. Charles Bailey.

December 11*th*, 1866.—"Notes on Wood-feeding Coleoptera." by Mr. Joseph Sidebotham.

December 13*th*, 1866.—"On a New Frame for Printing Transparencies on Glass," by Mr. Coote.

January 3*rd*, 1867.—"On the Mean Weekly Temperature at Old Trafford, Manchester, for the Seventeen Years 1850–1866," by G. V. Vernon, F.R.A.S., F.M.S.

January 3*rd*, 1867.—"On the Rainfall for 1866, at Old Trafford, Manchester," by G. V. Vernon, F.R.A.S., F.M.S.

January 3*rd*, 1867.—"Results of Observations of Variable Stars, made at the Mannheim Observatory during the year 1866," by Dr. E. Schönfeld, Director of the Observatory.

January 7*th*, 1867.—"On Polymorphina Tubulosa," by Thomas Alcock, M.D.

January 8*th*, 1867.—"On Two Remarkable Fossil Insects from

the Lower Coal Measures near Huddersfield," by E. W. Binney, F.R.S., F.G.S.

January 8th, 1867.—"On the Amount of Carbonic Acid contained in Sea Air," by Mr. T. E. Thorpe.

January 8th, 1867.—"On the Amount of Carbonic Acid contained in the Atmosphere of Tropical Brazils during the Rainy Season," by Mr. T. E. Thorpe.

January 10th, 1867.—"On 'Braun's' Photographic Copies of Drawings of the Old Masters," by J. B. Dancer, F.R.A.S.

January 10th, 1867.—"On Mr. Mercer's Process of Printing Photographs in various Colours," by J. B. Dancer, F.R.A.S.

January 22nd, 1867.—"On Casting, Grinding, and Polishing Specula for Reflecting Telescopes (Part II.)," by James Nasmyth, C.E., Corresponding Member of the Society.

January 22nd, 1867.—"On Storm Warnings," by Dr. Buys Ballot.

January 31st, 1867.—"Results of Raingauge, Anemometer, and Ozone Observations, made at Eccles, near Manchester, during the year 1866," by Thomas Mackereth, F.R.A.S., F.M.S.

January 31st, 1867.—"Observation of the Occultation of Aldebaran, January 16th, 1867, at Mr. Worthington's Observatory," by Joseph Baxendell, F.R.A.S.

January 31st, 1867.—"Observation of the Occultation of Aldebaran, January 16th, 1867," by Alfred Brothers, F.R.A.S.

January 31st, 1867.—"On the Elements of the Variable Star R Persei," by Joseph Baxendell, F.R.A.S.

January 31st, 1867.—"On the Variable Star R Vulpeculæ," by George Knott, F.R.A.S.

February 5th, 1867.—"On Barometric Waves," by Sir J. F. W. Herschel, Bart., M.A., F.R.S., &c., Honorary Member of the Society.

February 19th, 1867.—"On Siemen's and Wheatstone's Magneto-electric Machines," by Mr. Henry Wilde.

February 19th, 1867.—"On the Origination of the System of Issuing Storm Warnings," by Mr. T. H. Babington.

February 19th, 1867.—"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes (Part III.)," by James Nasmyth, C.E., Corresponding Member of the Society.

February 20*th*, 1867.—"On the Reversed Action of Light in Photography," by Mr. Joseph Sidebotham.

March 5*th*, 1867.—"On a Section of the Drift and the Underlying Triassic and Coal Measures at Ardwick," by E. W. Binney, F.R.S., F.G.S.

March 5*th*, 1867.—"On Some of the Conditions of Molecular Action," by Arthur Ransome, M.B., M.A., Cantab.

March 12*th*, 1867.—"On Photographs taken during the Eclipse of the Sun, March 6th, 1867," by Alfred Brothers, F.R.A.S.

March 19*th*, 1867.—"Observations of the Eclipse of the Sun, March 6th, 1867," by J. B. Dancer, F.R.A.S.

March 19*th*, 1867.—"On an Apparatus for Determining the Horizontal Magnetic Intensity in Absolute Measure," by J. P. Joule, LL.D., F.R.S., &c., V.P.

March 19th, 1867.—"Note on the Tangent Galvanometer," by J. P. Joule, LL.D., F.R.S., &c., V.P.

March 19*th*, 1867.—"On a Section of the Drift Deposits in the Banks of the Ribble, near Balderston Hall," by Edward Hull, B.A., F.G.S.

April 2*nd*, 1867.—"On the Microscopical Examination of Coal Ash or Dust from the Flue of a Furnace, Illustrated by the Microscope," by J. B. Dancer, F.R.A.S.

April 2*nd*, 1867.—"On the Galvanometer," by William Jack, M.A., Professor of Natural Philosophy at Owens College.

April 2*nd*, 1867.—"Additional Note on the Galvanometer," by J. P. Joule, LL.D., F.R.S., &c., V.P.

April 2*nd*, 1867.—"Notes on the Smallest and Simplest Forms of Matter and its Relations to Mechanical Forces," by Mr. J. C. Dyer, V.P.

April 2*nd*, 1867.—"Variable Star Ephemeris for 1867, calculated from the Elements given in Dr. Schönfeld's Catalogue," by J. R. Hind, F.R.S., Superintendent of the "Nautical Almanac," and Honorary Member of the Society.

April 9*th*, 1867.—"Note on Photography in 1787," by Alfred Brothers, F.R.A.S.

April 16*th*, 1867.—"On a New Form of the Dynamic Method for Measuring the Magnetic Dip," by Sir William Thomson, M.A., D.C.L., F.R.S., &c., Honorary Member of the Society.

April 16*th*, 1867.—"Further Remarks on the Galvanometer," by William Jack, M.A., Professor of Natural Philosophy at Owens College.

April 16*th*, 1867.—"Observations on the Alteration of the Freezing Point in Thermometers," by J. P. Joule, LL.D., F.R.S., &c., V.P.

April 16*th*, 1867.—"On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes (Part IV.,)" by James Nasmyth, C.E., Corresponding Member of the Society.

April 22*nd*, 1867.—"On Foraminifera," by Mr. James Linton.

April 22*nd*, 1867.—"On Ashes from Furnace Flues," by Mr. Joseph Sidebotham.

Many of these papers will appear in the forthcoming volume of Memoirs, the printing of which is far advanced, and is expected to be completed before the commencement of the ensuing session.

The system of admitting Sectional Associates has continued to work satisfactorily, and the Council have therefore resolved to recommend its renewal for another year.

The Librarian reports that the Society continues to be in regular receipt of the usual Transactions, Memoirs, &c., issued by the various learned bodies with which the Society is in correspondence. The number of societies, &c., exchanging their publications with this Society is now 256, 74 of which are British and 182 foreign, or an increase of 5 British and 7 foreign upon the number of last year.

Since the date of the last Report the attention of the Librarian has been chiefly given to the acquisition of volumes, parts, &c., which have been wanting for some time past, and it is gratifying to be able to state that unusually large and valuable donations of such desiderata have been added to the Library in the course of the past year. The Philosophical Transactions for 1845 and 1846, referred to in Vol. III. of Proceedings, p. 278, as having been lost, and many other Journals, &c., entered in the catalogue as wanting, have been replaced in this way.

The number of works accumulating in the Library has necessitated the enlargement of one of the book-cases, and the time is not distant when additional space will be required for the increasing number of publications which are coming to hand daily.

The following is a list of serial publications which the Librarian is authorized to purchase for the Library :—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Annals and Magazine of Natural History.
The Quarterly Journal of Science.
The Zoologist.
The Publications of the Palœontographical Society.
„ „ „ Ray Society.
„ „ „ Cavendish Society.
The Quarterly Journal of Pure and Applied Mathematics.
Les Annales de Chimie et de Physique.
Les Annales des Sciences Naturelles ; Zoologie et Botanique.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Prodromus systematis universalis regni vegetabilis, par Alphonso de Candolle.
Die Astronomische Nachrichten.
Die Annalen der Physik und Chemie.
Die Annalen der Chemie und Pharmacie.
Die Journal fur die reine und angewandte Mathematik.
Neues Jahrbuch fur Mineralogie, Geologie, und Palœontologie
Die Zeitschrift fur wissenschaftliche Zoologie.
Allgemeine Encyklopädie der Physik.

On the motion of Mr. CARRICK, seconded by Mr. BROTHERS, the Annual Report was unanimously adopted.

The following gentlemen were elected officers of the Society for the ensuing year:—

President.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.

Vice-Presidents.

ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S., &c.
JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., &c.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
JOSEPH CHESBOROUGH DYER.

Secretaries.

HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.

ROBERT WORTHINGTON, F.R.A.S.

Librarian.

CHARLES BAILEY.

Other Members of the Council.

REV. WILLIAM GASKELL, M.A.
PETER SPENCE, F.C.S., M.S.A.
GEORGE VENABLES VERNON, F.R.A.S., F.M.S.
JOHN BENJAMIN DANCER, F.R.A.S.
WILLIAM JACK, M.A.
WILLIAM LEESON DICKINSON.

ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER,

Dr. FROM 1ST APRIL, 1866, TO 31ST MARCH, 1867. Cr.

1866.	£	s.	d.	£	s.	d.
April 1.—To Balance in Bank of Heywood Brothers and Co.	267	2	3			
,, ,, ,, Treasurer's hands	2	2	0			
1867.				269	4	3
Mar. 31.—To *Members' Contributions:*						
Arrears, 1864-5 ... 1..	2	2	0			
,, 1865-6 ... 18..	37	16	0			
,, Half Subscription, 1866 ... 1..	1	1	0			
,, Admission Fees, 1866 ... 3..	6	6	0			
Members on the Roll, April 1, 1866, 183 at 42s. .. £384 6 0						
Deduct Compounders ... 3						
,, Honorary Member ... 1						
,, In Arrear, March 31, 1867 ... 18 — 22 — 46 4 0						
	338	2	0			
Three Members elected in 1866, April—December	6	6	0			
Three Admission Fees	6	6	0			
Two Readmissions	12	12	0			
Eleven Associates of Microscopical Section, at 10s.	5	10	0			
Two ditto Arrears ditto at 10s.	1	0	0			
Eight ditto of Photographic Section, at 10s. 6d.	4	4	0			
One ditto of Physical and Mathematical Section, at 10s. 6d.	0	10	6			
				421	15	6
,, ,, To *Sale of Publications:*						
Memoirs—Ser. 2, Vol. XV.	0	7	6			
,, Ser. 3, Vol. I.	0	15	9			
,, Ser. 3, Vol. II.	0	17	6			
,, Ser. 3, Vol. VIII.	0	7	0			
Proceedings	1	2	4			
Memoir of Dalton	0	3	6			
				3	13	7
,, ,, To *Sundry Income:*						
Sec. Contrib. Microscopical ... £4 4 0						
,, ,, Photographic ... 2 2 0						
,, ,, Physical and Mathematical ... 2 2 0						
	8	8	0			
,, ,, Interest allowed by Bankers	13	6	4			
				21	14	4
				£716	7	8
Composition Fund ... £72 10 0						
General Balance ... 250 2 1						
	£322	12	1			

Mar. 31, 1867.	£	s.	d.	£	s.	d.
By *Charges on Property:*						
Chief Rent	12	14	0			
Fire Insurance	5	12	6			
Property Tax	2	16	8			
Inhabited House Duty	6	7	6			
				27	10	8
,, *House Expenditure:*						
Water, Gas, Candles, and Coal	19	2	5			
Cleaning and Petty Expenses	2	15	9			
Bookcase and Furniture renewed, &c.	26	11	0			
Repairs	2	17	3			
Tea and Coffee at Meetings	17	12	8			
				68	19	1
,, *Administrative Charges:*						
Wages—Keeper of Rooms	49	4	0			
Attendance on Sections	8	8	0			
Postage and Parcels	29	0	9			
Stationery, Printing Circulars, &c., and Receipt Stamps	10	12	6			
				97	5	3
,, *Publishing:*						
Memoirs—Printing, Engraving, &c.	80	10	0			
Printing Proceedings	48	7	0			
Editor of Memoirs and Proceedings	50	0	0			
				178	17	0
,, *Library:*						
Periodicals, &c.	18	16	7			
Subscription to Palæontographical Society	1	1	0			
,, Ray Society	1	1	0			
				20	18	7
,, *Sundry Expenditure:*						
Returned Honorary Member for Vol. of Memoirs charged in error				0	5	0
Total Disbursements				393	15	7
Balance in Bank of Heywood Brothers and Co.	314	9	1			
,, ,, Treasurer's hands	8	3	0			
				322	12	1
				£716	7	8

2nd April, 1866.

ROBERT WORTHINGTON, TREASURER.

Examined and found correct, 12th April, 1867.

GEO. CLIFF LOWE,
W. L. DICKINSON, } AUDITORS.

"On the Galvanometer" by WILLIAM JACK, M.A., Professor of Natural Philosophy at Owens College.

The author, referring to his communication entitled "Further Remarks on the Galvanometer," printed in the last number of the Society's Proceedings, points out that a serious misprint had occurred on page 159, where in the third line $n+i$ should be n and i. He then suggests that the use of the formula he had given would afford a very satisfactory method of determining the magnetic length of a bar magnet. Thus for $n=\frac{213}{315}$, we should have a negative result for any intensity between such as will make $\sin^2 a_1=\frac{105\cdot3}{213}$ and $\sin^2 a_1=\frac{116\cdot7}{213}$. Thus, if the very first entire length of magnet for which this reversal of effect takes place (trying all possible intensities) be L, since $\frac{l^2}{r^2}>\frac{212\cdot1}{315}$ and $\frac{L^2}{r^2}=a$, a known fraction $\frac{l^2}{L^2}$ must be $>\frac{212\cdot1}{315}\frac{1}{a}$, and as I have shown above that it is probably very possible to obtain by sufficient care instead of the vague $>\frac{212\cdot1}{315}$ something which is at least between $\frac{212\cdot1}{315}$ and $\frac{213}{315}\frac{l^2}{L^2}$ can be determined with great accuracy and $\frac{l}{L}$ will lie between $\sqrt{\frac{212\cdot1}{315}\cdot\frac{1}{a}}$ and $\sqrt{\frac{213}{315}\cdot\frac{1}{a}}$. Probably in practice much narrower limits might be found determinable, with ordinary values of i. Thus even for $n=212\cdot2, x=\sin^2 a$ would lie between $\frac{111\cdot13}{212\cdot2}$ and $\frac{110\cdot33}{212\cdot2}$ which gives a fair range for a, so that it is unlikely that a good experimenter, carefully raising or lowering the intensities, would fail to observe the change in sign of deviation which corresponds.

STORM WARNINGS.

Mr. BAXENDELL, in reply to a question from Mr. Brothers, F.R.A.S., stated that General Sabine had not yet offered any reply to the letter he had addressed to him on the 26th March commenting upon the proceedings of the President and Council of the Royal Society in reference to the Meteorological Department of the Board of Trade and the suspension of storm warnings. It was evident that these proceedings would not bear examination and discussion, and as a consequence, no confidence could now be placed in the so-called "Scientific Committee," which had been appointed by the President and Council, to superintend the Meteorological Department. This committee had shown itself to be utterly regardless of public opinion and feeling, and quite unfitted to carry out efficiently the duties which had been so ably and so usefully discharged by the late Admiral Fitzroy and his assistant Mr. Babington; and in the interests of science, commerce, and humanity it was to be hoped that the Board of Trade would again take the management of the Meteorological Department into their own hands, and appoint Mr. Babington, or some other competent and responsible person, to resume and carry on the system of storm warnings.

As an illustration of the lamentable consequences of the suspension of storm signals it might be stated that among the wrecks caused by the unusually heavy gale of the 10th of April there were four vessels which had sailed from Liverpool just before the commencement of the storm. Three of these vessels had been thrown on the Lancashire coast and totally lost, and the lives of three of the crew of one vessel had been sacrificed. The fourth vessel had foundered in the Irish Sea and all hands were lost. Now the meteorological phenomena immediately preceding the occurrence of this storm were of such a nature that there could have been no difficulty in giving notice of its approach in ample

time to prevent the sailing of these vessels. In fact, without the aid of telegrams from distant stations, its approach was confidently announced in Manchester so early as the evening of Monday the 8th of April, and the direction in which the wind would blow during the most violent period of the storm was also stated at the same time. The grounds of this prediction were explained, and it was shown that even if the rules of meteorology were not yet established "on a strictly scientific basis," the plea put forward by the President and Council of the Royal Society for discontinuing storm signals could only be regarded as frivolous. It is understood that the expense of the Meteorological Department, under the new management, is now more than double the amount it cost under the management of the late Admiral Fitzroy, and yet the most important and practically useful portion of its duties, and that which involved the greatest current expense, has been given up; and while even the Scientific Committee cannot deny that our present knowledge of the laws of meteorological phenomena affords us the means of saving immense amounts of valuable property and many valuable lives, yet this committee has nevertheless presumed to deprive the public of the advantages to be derived from this knowledge, and has not hesitated to divert the funds which have hitherto been so usefully applied in the interests of commerce and humanity, to the furtherance of schemes and scientific crotchets which are altogether uncalled for by the requirements of meteorological science, and which, however interesting and important they may be to the parties immediately concerned, have certainly no interest whatever for the general public. Surely this is a state of things which calls for the interference of those who are charged with the power and responsibility of controlling the expenditure of the public money.

Mr. Baxendell also called attention to the increasing number of vessels which were reported as having foundered at sea, and Mr. Binney suggested that it might be owing to the greatly increased length of modern built ships, especially those constructed of iron.

Mr. W. B. Johnson said, the fact referred to by Mr. Baxendell admitted of easy explanation. The materials now used in the building of iron ships were greatly inferior, both in strength and quality, to those used in ships built some years ago. In some cases, where a builder formerly employed iron plates three-eighths of an inch thick he now contented himself with plates only three-sixteenths of an inch in thickness, and the strength of the angle-irons, &c., was reduced in the same proportion. It was no exaggeration to say that the weight of the iron now used in building a vessel of a given tonnage was fully one-third less than was generally considered necessary some years ago; and besides this, the iron now used for ship's plates was notoriously of very inferior quality. No recently built iron ship could be found that would bear the bumping and straining which the "Great Britain," built a quarter of a century ago, bore in Dundrum Bay.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 25th, 1867.

A. G. Latham, Esq., President of the Section, in the Chair.

Mr. Coward exhibited specimens of Calluna vulgaris from Newfoundland, with the following memorandum, in the handwriting of the late Robert Brown, on a ticket attached: "Erica vulgaris Linn. at the head of Trepassey and St. Mary's Bay on the south-east coast of Newfoundland, covering considerable tracts. Mr. Mc. Cormack received from Mr. George Don, June 3rd, 1830. In the collection were three specimens, none of them in fructification.—R. B."

Dr. Seemann (*Jour. Bot.*, Oct., 1866) thinks the Newfoundland Heath may prove to be a distinct species, but Dr. Asa Gray considers it identical with the British one. These specimens favour the latter view as far as foliage is concerned.

The Rev. J. E. Vize exhibited and described the hairs of a foreign Lepidopterous caterpillar. He said the specimen had remained for many years in spirit, and no record had been preserved with it; it was evidently not British, but he had hitherto been unable to identify it with any described foreign species in books to which he had access.

The body was covered with very large compound hairs which gave the entire creature the appearance of a tuft of moss, and might possibly serve for its protection by the likeness to a vegetable growth. The unusual and peculiar

development of these hairs suggested a microscopical examination of them, and the result had been interesting as showing that, whether or not the entire mass of the compound hairs serve for protection by concealment, the complex structure of their points, which are capable of protrusion and have each a bag or gland at their base contained within the tubular branch from which they project, fits them to act as most efficient weapons of offence.

March 25th, 1867.

J. SIDEBOTHAM, Esq., in the Chair.

Mr. HURST said he had received a letter from Mr. R. G. BREAREY, dated February 16th, Douglas, Isle of Man, referring to three living specimens of Echinis sphæra, kindly sent by him, and received here, in good condition. The attempt had been made to keep them alive in artificial sea water, but after several days they appeared to be dying, and were put in spirit. They are now at the service of any members who wish to have them for microscopic examination.

Mr. Brearey mentions in his letter that he had asked the man who procured the specimens for him what he knew of the habits of the Echini, and was told that they walk by by means of their spines, and feed upon any animal matter living or dead; also that they are sometimes eaten boiled, and taste like the roe of the codfish.

The thanks of the meeting were voted to Mr. Brearey for his communication and for the accompanying specimens.

Mr. DANCER, F.R.A.S., read a paper "On the Microscopical Examination of Coal Ash or Dust from the Flue of a Fur-

nace, illustrated by the Microscope." [This paper was afterwards read at the ordinary meeting of the Society, held April 2nd, 1867. See page 143.]

Mr. C. BAILEY exhibited the curious fruits of Pawlownia imperialis, really a native of Japan, but found wild in the woods near New York. Mr. Bailey presented a specimen slide to the Section.

Mr. WATSON read a paper entitled "Further Remarks on the Plumules or Battledore Scales of some of the Lepidoptera, with illustrations by Mr. Sidebotham."

April 22nd, 1867.

ARTHUR G. LATHAM, Esq., President of the Section, in the Chair.

Mr. SIDEBOTHAM exhibited and described specimens of coal ash from the furnace flues of the Strines Printing Company, to which his attention had been drawn by Mr. Dancer's paper.

They consisted in a great part of spherical bodies almost exactly simulating "shot and shell" on a small scale, the aperture for charging the shell bearing a singular resemblance to that in real shell; some of the "shot" were almost the size of dust shot. He stated that many cwt. were annually carted away from these flues, and suggested that a small iron mine had been discovered.

Although under examination by the microscope these bodies reflected distant objects very perfectly, especially when slightly above the focus, their surfaces were much corrugated, and the high polish they apparently possessed

when out of focus was shown to be entirely illusive when the surface was accurately examined.

Mr. NASMYTH, to whom he had sent specimens, compared these bodies to the planets (on the nebular hypothesis) thrown off centres of intense heat.

Mr. LINTON read the following paper "On a sample of sand from Dogs Bay, Connemara, skimmed from the surface of the sea by Mr. Glover, in July, 1866."

While examining various samples of sand brought at different times from Dogs Bay it has frequently occurred to me that each packet is very perceptibly different from the others in the forms of Foraminifera which preponderate, and that, while certain forms are common in some of the samples, they appear to be altogether absent from others. I have very little doubt that these differences depend on the time of year at which such gatherings were made, and whether they were taken from the shore or skimmed from the surface of the sea, those obtained by the surface skimming always yielding the most perfect specimens. The samples which I had before examined were none of them at all to be compared for richness with that obtained by the late Mr. Parry by skimming the surface of a piece of water on the higher part of the shore, and to which he gave the name of Burns' Pool. I have prepared a slide containing all the forms found in it, and this I have now brought for exhibition.

Mr. Glover kindly collected fresh samples of sand for me in July last, and again in December. The latter gathering was from the shore, and though it contained a great variety of objects, the more delicate of them were in many cases broken or worn. The July sample, obtained by skimming from the surface of the sea, and unfortunately procurable only in very small quantity, is that to which I wish particularly to draw your attention, as it consists of more perfect

and beautiful specimens than I have seen before. I now show these specimens mounted, and present the following list of them. Before concluding I would remark, that it is very desirable for members examining Foraminifera to keep a record, not only of the place, but of the time of year, when their sand was collected.

List of Foraminifera from Dogs Bay.

Lagena vulgaris typica. None.
„ „ var: antiqua. Very frequent.
„ „ var: clavata. Not common but very perfect.
„ „ var: perlucida. Very fine.
„ „ var: semistriata. Very fine and perfect.
„ „ var: striata } ... { Very common, several double and many deformed specimens.
„ „ var: interrupta }
„ „ var: gracilis. None.
„ „ var: substriata. Very fine; several varieties, among them one with the striæ extending spirally up the neck.

Entosolenia globosa var: lineata. Not common.
„ costata. Common.
„ marginata. Very plentiful.
„ Williamsoni. Very plentiful.
„ squamosa typica. Very plentiful and much varied in form.
„ „ var: scalariformis. Not common.
„ „ var: catenulata. Not common.
„ „ var: hexagona. Common.

Lingulina. None.
Nodosaria radicula. Occurs sparingly, but perfect.
„ pyrula. None.
Dentalina. None.
Frondicularia. None.
Cristellaria subarcuatula. Rare; no other form of Cristellaria occurs.

Nonionina Jeffreysii. Rather common.

Polystomella crispa. Very common and fine. Some specimens though large have spines like those of the young shell; the same is the case with specimens I have obtained from sand from the Dardanelles.

„ umbilicatula. Rare.

Peneroplis. None.

Patellina corrugata. Small, but very perfect.

Rotalina. The sand contains all the varieties excepting ochoracea, inflata and turgida. R. oblonga is fine and plentiful and is very varied in form.

Globigerina bulloides. Common.

Orbulina universa. Very large.

Planorbulina vulgaris. Very plentiful.

Truncatulina lobata. Common.

Bulimina pupoides var: marginata. Rare.

Uvigerina. None.

Cassidulina. Not common.

Polymorphina. All the varieties plentiful.

Textularia cuneiformis. Not common.

Biloculina ringens var: carinata. Rather common.

Spiroloculina depressa. Frequent.

Miliolina. All the varieties.

Spirilina. None.

Dr. CARRINGTON read the following paper "On two Hepaticæ New to Britain."

Jungermannia saxicola, Schrad.

Stems ascending, rigid, dichotomously branched, sparingly radiculose, sub-terete, examphigastriate.

Leaves bifariously imbricated, uniform, unequally bi-lobed, conduplicate, inferior lobe rotundate ovate, obtuse, or shortly pointed, concave, arching forwards; sinus deep, gibbous; dorsal lobe smaller, incumbent, cuneate, clasping the stem. Fruit terminal, or from the growth of innovations axillary,

involucral leaves 2-4-fid, lobes unequal, repand-serrate; perianth ovate, 6-plicate, the apex lacinulate-dentate.

J. saxicola, Schrader Sammlung cryptogamischer Gewächse 1796-7, Syn. Hepat. p. 119.

J. resupinata, Lin. Fl. Suecica, ed. 1, 1745 (excl. syn. Dillen.)

This fine species was discovered June, 1864, at Ronas Hill, Shetland, by A. Mc. Kinlay, Esq. In general habit J. saxicola resembles J. minuta, but the leaves are more closely imbricated, the anterior lobe smaller, and the posterior curved forwards, so that the back of the frond looks terete. The leaves are of firm texture, olive-brown, scarcely altered when dry, areolation dotted.

J. saxicola is not unfrequent in Norway and Sweden, and is also found in alpine districts through Europe; so that I have long looked for it from Scotland. Some years ago the late Dr. Greville was kind enough to lend me his duplicates of Hepaticæ, and in a paper with J. orcadensis from the Southerland mountains, I found two stems of J. saxicola, as if they had been picked out of some tuft; but as there were European specimens in the same packet, I thought they might have got there accidentally.

The synonymy of J. saxicola was long in a confused state. Linnæus, in the Flora Suecica, confounded it with J. resupinata *Dillenius*, which was probably Scap. œquiloba.

S. capania Bartlingii N. ab E.

Plagiochila Bartlingii M. et N. Hep. Eur. III. p. 520.

Stems short, creeping at the base.

Leaves patent, conduplicate, amplexicant, the lobes narrowly ovate, entire, cuspidate, those of the upper leaves obtuse, sinus shallow, gibbous, margin undulate.

Perichdyætium obovate, compressed, mouth truncate, entire.

J. cuspiduligera N. ab E. Hep. Eur. I. p. 180.

Stems ½in. to 1in. high, fasciculately branched, leaves pale green, the lobes equal, both short, with acute points, the sinus reaches only about ¼th of the leaf, base of the leaf clasping the stem, margin entire, repand-undulate, and looking somewhat crisped when dry.

I first collected this species April, 1858, by the Strid, Bolton Woods, Yorkshire, and more recently have received specimens from Mr. Mudd, from Teesdale.

In general habit J. Bartlingii resembles some aquatic forms of S. arcosey, Ehrharti (J. emarginata), more than a S. capania. The lobes are more nearly equal, and the sinus shallower, than in any other species with which I am acquainted, and are not recurved.

From J. æquiloba it may be distinguised by the entire leaves—those of J. æquiloba are coarsely toothed, and the anterior lobe, except in the apical leaves, smaller than the under one.

From S. compacta it may be known by the lobes in that species being nearly round,—whereas in J. Bartlingii they are ovate, acute, and undulate.

Dr. Alcock read a paper "On the hair of the Musk-deer."

He said that he found no full account of it in the books he had referred to, though most of its characters were separately mentioned by different authors.

The hair is about two inches long, thick, rigid, brittle, without elasticity, and scarcely at all flexible; it has nearly a uniform thickness for three-fourths of its length from the base, and then gently tapers to a fine point; its section is not quite circular but slightly flattened on two sides, the other sides having a wavy outline in consequence of the hair being bent alternately in opposite directions; in the length of the hair there are about fourteen of these bendings, they are most strongly marked in the middle third, at which

part the extent of the alternate lateral divergences is about equal to the thickness of the hair, but they are absent for some distance both from the base and tip. The bulb is glassy and transparent, showing no structure, flask-shaped, and joined to the body of the hair by a rather long, slender, transparent neck. All this glassy part is embedded in the skin, and, at the point where the hair projects beyond, it suddenly swells out to its full thickness, forming a hemispherical base. A remarkable character of these hairs is their singular lightness, which may be seen by letting them fall from the hand, when they float through the air like feathers. With the exception of the bulb, the entire hair has a cellular structure, and what might be called its cortical portion is nothing more than a surface-layer of small polygonal cells without any appearance of horny substance, while all the inner parts consist of a very coarse cellular structure in which no regularly-shaped cells can be seen, but there is rather the appearance of shapeless cavities separated by very thin membranous partitions. This inner structure is so delicate that the hair can be crushed in and flattened by very slight pressure, though there can be no doubt that its strength is greatly increased by the wavy form already mentioned, this being an arrangement similar to the ridge-and-furrow of iron structures, which is so important an element of their strength. Dr. Carpenter, in his work on the Microscope, mentions that the hair of the Reindeer has a very similar structure to that of the Musk; it appears, in fact, to be identical in plan, and this complete cellular structure, so different from that of ordinary hair, is found not only in the Reindeer, but in the common Stag, the Rusa Deer, and the Roebuck, that of the Roebuck showing the nearest approach in general character to the hair of the Musk-deer. With regard to the Caribou, which may be presumed to have hair of similar structure to that of the European Reindeer, the statement of Dr. Richardson that

the skin dressed with the hair on forms a clothing so impervious to cold that a person dressed in a suit of this material and wrapped in a mantle of the same, may safely pass the night in the snow during the intensity of an arctic winter, throws light on the importance of this cellular hair to animals fitted for living in cold climates and at the same time having a constitution which requires them to be provided with a very warm external covering. The whole interior of these cellular hairs is occupied by air, which, as is well known, forms the best possible non-conductor of heat, and, in considering their efficiency for this purpose, the manner in which they are implanted in the skin should be noticed. In the Musk-deer there appears to be absolutely no fine wool or under-fur on the back and sides (which were the only parts examined), and the thick cellular hairs are so closely set in the skin that they necessarily stand out from it like the spines of a hedgehog, the consequence being that the thickness of the warm covering is not much less than the entire length of the hairs; and it should be further noticed that their effect is increased by the uniform thickness which they preserve for so great a proportion of their length, and also by the wavy form already described, which allows them to fit, as it were, to one another.

In the Roebuck there are a few slender hairs having the ordinary appearance of under-fur scattered among the large, rigid, cellular hairs which are seen standing out like quills; these latter are of the same length as in the Musk, though not quite so thick, but they are still so closely set in the skin that they form a warm covering on the same principle. In the true Deer which have been found to possess these wavy cellular hairs the perfect development, as seen in the Musk, is further departed from, and they become longer in proportion to their thickness, wiry, and more scattered, and at the same time mixed with a greater quantity of under-fur.

The PRESIDENT stated that, in addition to the Foraminifera found in Dogs Bay Sand, he had noticed the following objects:

Seeds.
Parts of Insects.
Echini spines, disks, pedecellariæ and shell.
Portions of Zoophytes.
Opaque and transparent globes not yet identified.

Mr. SIDEBOTHAM exhibited fossils from the plateau of the Great Pyramid, among them a very perfect Echinus.

Dr. ALCOCK exhibited a number of bones from a cave in North Wales, which, on examination, he had identified as belonging to the common frog.

A Letter was read from Mr. HEPWORTH, accompanying lithographs of the magnified Male Itch Insects and Cancer Cells, for distribution among the members, and the thanks of the meeting were accorded to him.

Annual Meeting, May 6th, 1867.

The Annual Report having been read by the Secretary, was unanimously adopted, and ordered to be printed in the usual form and circulated among the members.

The following Officers were elected for the ensuing Session:—

President:

JOHN B. DANCER, F.R.A.S.

Vice-Presidents:

ARTHUR G. LATHAM.
JOSEPH SIDEBOTHAM.
JOSEPH BAXENDELL, F.R.A.S.

Secretaries:

HENRY ALEXANDER HURST.
THOMAS ALCOCK, M.D.

Treasurer:

JAMES G. LYNDE, F.G.S., F.R.M.S., M.Inst.C.E.

Of the Council:

W. C. WILLIAMSON, F.R.A.S.
JOHN WATSON.
ALFRED BROTHERS, F.R.A.S.
SAMUEL COTTAM.
CHARLES BAILEY.
THOMAS NEVILL.
E. W. BINNEY, F.R.S., F.G.S.
THOMAS COWARD.

The Rev. J. E. VIZE read a paper "On Testacella."

He described the general appearance of this slug, and gave an account of its habits as he had observed them in the neighbourhood of Trowbridge, where it is common. He also exhibited several mounted tongues of Testacella, and pointed out the peculiar characters of the teeth.

July 18th, 1867.

J. B. DANCER, F.R.A.S., President of the Section, in the Chair.

"Some Further Observations on the Cause of Rotation in the Cells of Vallisneria," by JAMES G. LYNDE, F.G.S., F.R.M.S.

In a paper read by me at a meeting of the Section on the 16th February, 1863, "On the Action of Magenta Dye upon Vegetable Tissue," I described a series of experiments upon cuttings of Vallisneria, made chiefly with a view to ascertain, if possible, the cause of the rotation of the chlorophyl vescicles within the cells.

I then concluded that the rotation was due to the action of cilia on the inner surface of the cell wall, and was con-

firmed in this opinion not only by the appearance of the luminous stratum or so-called ciliary wave which had been observed by Dr. Branson, Mr. Wenham, and others, but also by the appearance on the cell walls of certain markings revealed by the action of the dye on the suspension of the vital action.

The above experiment was exhibited to the Section, and many of the members present attributed these markings, as I had done, to the presence of cilia.

I have since, from time to time, pursued my experiments on the subject in the hope that I might be able to adduce more positive evidence as to the cause of the wave of light on the interior of the cell wall.

After many fruitless experiments I at length determined to try the effect of polarized light, and on the application of it with a $\frac{1}{8}$th-inch objective, having an aperture of 130° to 162°, and so arranging Darker's series of selenite plates as to give a dark blue ground, there appeared over the surface of all the cells brilliant gold-coloured scintillations which had all the appearance of cilia in motion.

The portion of leaf under examination exhibited very sluggish circulation, and was therefore in a very favourable state for the observation.

I have since repeated the experiment several times, and have never failed witnessing the same appearance.

Notwithstanding all that I have seen I cannot say that I am convinced the appearances can be attributed to nothing else but cilia; it is possible they may be due to the presence of active corpuscles, as suggested by Mr. Wenham in his paper on the leaf cells of Anacharis alsinastrum published in the *Microscopical Journal* for 1855, which corpuscles may be Vibrionia or Zooglæa, described by Dr. Cohn in his "Researches on the Development of the Microscopic Algæ and Fungi," as representing the developmental condition of a plant, but it is only by further research that this point can be definitely settled.

The result of my observations so far appears to be that in addition to the wave of light already seen, the separate objects causing that wave may now be observed in the manner I describe; what these objects are is still a matter to be determined, but at present I am inclined to believe them to be cilia on the cell wall, while at the same time there are also independent moving corpuscles within the cell; some of these bodies have the appearance of crystals, and in one specimen I observed a great number of starch granules in the cells.

In investigating this subject the smallest step in advance cannot but be deemed of importance, and I trust that in giving the results of my observations as they occur, I may be the means of saving time and trouble to others who may be investigating the same class of objects and of inducing them to follow up so interesting a subject, as it is only by many and independent observations the truth can be ascertained.

For the information of members I may state in detail the method of observation I have found most successful.

The microscope I made use of is one of Smith and Beck's largest sized binocular, with one of Beck's most recent $\frac{1}{8}$th-inch objectives.

The illumination was by means of an Argand gas burner, the light passing into a right-angled prism below the stage in a line with the axis of the object glass.

Immediately beneath the stage was the achromatic condenser, used both with and without the central stop in the diaphragm, the object being seen equally well in both ways with different effects of light.

Below the achromatic condenser was fixed Darker's series of selenites, and below this the polarizing prism, the analysing prism being inserted immediately above the objective.

I made use of the low Huyghenian eye-pieces, and used the microscope either as a binocular or single tube, the field being well illuminated in each case.

I need not add that the most careful centering and adjustment are essential.

In preparing the object I made a section of a small portion of the leaf laid on a piece of cork in water under a simple microscope, separating only one layer of cells; I laid this on a slip made of thin covering glass, and over it applied a thin glass cover, a small feeding bottle and thread being made use of to prevent the object being dried by evaporation.

I then covered the stage and object with a piece of black velvet, to prevent interference from other lights in the room.

Mr. SIDEBOTHAM reported the plentiful occurrence, in June, of Thlaspi arvense, L., on the borders of Carrington Moss. It has very rarely been noticed in the neighbourhood of Manchester.

ON THE ABSORPTION OF GASES BY CHARCOAL.

The following Letter from Dr. R. ANGUS SMITH, F.R.S., to Dr. J. P. JOULE, F.R.S., was received by the Editor, June 20th, 1867.

Manchester, June 17th, 1867.

My Dear JOULE,

You asked me about my experiments on the absorption of gases by charcoal. I certainly seem to delay them, but I have little spare time. In 1848 I illustrated the oxidising power of porous bodies, referring chiefly to sand. In 1862, when speaking of the absorption of gases by charcoal, I ventured to say that the physical and chemical action could not be separated. I have been anxious to obtain more direct evidence.

I had worked a good deal with the mixed gases, but lately thought it better to return to the simple, although unwilling to question results got by others. Five of these gases have been tried, and they are found to be absorbed by charcoal in whole volumes and not in fractions of a volume, hydrogen being taken as one. In three cases hydrogen, oxygen, and carbonic acid, the numbers are 1, 7·99 and 22·05 extremely exact volumes, with a relation the same as our ordinary atomic weights. Saussure's numbers treated thus give 5·3 and 20, that for nitrogen being 4·2.

It is only by taking the average of many experiments that these results have been obtained, but, in doing so, every one has been added without selection. The numbers from which the averages are obtained diverge so much that I suppose others have not thought of obtaining anything definite. This is caused by the difficulty of finding perfectly uniform charcoal.

I have not found the other numbers to be the same as the equivalent, although still whole. Equivalents promise here to enlarge their bounds. I cannot believe that these numbers can be the results of any accident. They must be distinguished from chemical equivalents by weight.

I am,

Yours sincerely,

R. Angus Smith.

J. P. Joule, Esq., L.L.D., F.R.S., &c.

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

MANCHESTER.

VOL. VII.

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NOTE.

The object which the Society have in view in publishing their Proceedings, is to give an immediate and succinct account of the scientific and other business transacted at their meetings, to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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ERRATA.

Page 194, line 23, for "L'Ancrene," read "L'Ancresse."
" " " 28, for "ophioglonifolius," read "ophioglossifolius."
" 195 " 6, for "Branica adprena," read "Brassica adpressa."
" 196 " 13, for "enincarnatum," read "eu-incarnatum."
" " " 15, for "Tm.," read "Ser."
" " " 21, for "thyrssidens," read "thyrsoideus."
" " " 28, for "Exholtzia," read "Escholtzia."
" " " 33, for "L'Ancrene," read "L'Ancresse."
" 197 " 9, for "parrifolius," read "porrifolius."
" " " 16, for "Imila," read "Inula."
" " " 17, dele "the."
" " " 32, for "Bonley," read "Bouley."
" 198 " 3, for "vernation," read "venation."
" " " 14 & 31, for "L'Ancrene," read "L'Ancresse."

PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 1st, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S, &c., President, in the Chair.

Mr. E. W. BINNEY, F.R.S., &c., said that in the *Dundee Advertiser* of the 12th ultimo a report is given of some remarks on *Calamiteæ* and fossil *Equisitaceæ*, by Mr. William Carruthers, of the British Museum, who stated "that the most important characters were obtained by botanists from the fructifications, and the author had obtained, through the kindness of Dr. Hooker, sections of vegetable structures prepared by Mr. Binney, whose extensive acquaintance with coal plants was well known. He had discovered in some of the fruits which belonged to *Calamites* so beautifully preserved that the most minute details could be determined with the help of his diagrams in the various points in which they agreed from the fruits of the recent *Equisitaceæ*."

Now some twelve years since the specimens said to be "prepared" by him (Mr. Binney) were discovered and immediately slided, polished, and sent up to London to a most eminent scientific man, who had agreed to join him (Mr. Binney) in publishing a description of them, and whose name it was not then necessary to give. With the specimens sent up was written, 29th November, 1854, as follows: "You will

be delighted to find such beautiful sections of *Volkmannia* in the large slide I now send. They are well worth the trouble and expense of the slide. These doubtless belong to *Calamites*." When he saw the same gentleman in London soon after he pointed out to him under the microscope the small spore-like bodies in the Sporangia.

At that time the whole of the structure of *Calamodendron* had been made out with the exception of the centre of the stem and the connection of *Volkmannia* with it. The valuable papers of Dr. Ludwig on *Calamite* fruits, and Dr. Geoppert on *Aphyllostachys* afforded him much information, but he obtained the evidence of *Volkmannia* being the fruit of *Calamodendron* from the similarity in structure of the central axes. About two years since he cleared up both the above points to his satisfaction, and then immediately wrote to the gentleman to whom he had sent his specimens and urged upon him the necessity of their immediately publishing. The latter replied that his engagements would not allow him to join, he having a short time previously returned the specimens which had been so long in his possession. He (Mr. Binney) then, unaided, lost no time in commencing to write a description of his specimens, which is now in the press and intended to be shortly published by the Palæontographical Society. Some of the plates were engraved a year ago, and nearly all of them were in the engraver's hands and would have been done had the author's health allowed. A few months since he thought the paper would be published in October, but owing to delays over which he had no control such has not been the case. All he asks is for the public to give him credit or discredit, as may be, for any discoveries he may make, as well as to the parties who have, without his consent, obtained possession of his specimens, even although the latter may anticipate him in publishing a few weeks. No doubt the observations are independent of each other—may differ in some respects

—and his may be less valuable than those of Mr. Carruthers; still it is not unreasonable that he should wish to be considered as an original investigator rather than a preparer of specimens for other people.

Dr. CROMPTON, alluding to the paper he read in October, 1866, "On the Portraits of Sir Isaac Newton," said, that while preparing his essay for publication in the Memoirs of the Society, he had opened up fresh sources of information, and become possessed of facts of considerable interest respecting the portraits of Newton. He had examined about twenty portraits of the great philosopher, all of which were considered to be originals, and most of which were undoubtedly painted a long time ago; but he had seen no portrait which, in his opinion, was of equal importance and interest with the Kneller Newton of 1689, to which he last year directed the attention of the Society, as by far the most valuable portrait of the great philosopher in existence, and of which he exhibited an admirable engraving by Mr. Oldham Barlow. His extended inquiries into the subject of the portraits of Newton had led him to the conclusion that there are several (if not many) which pass current as portraits of him which are most decidedly representations of other persons. In the National Portrait Exhibition at Kensington of the present year there were four portraits of Newton, two of which he feels sure have no real claim to be regarded as authentic or genuine. These two are pictures contributed by the Earl of Dartrey and by the Marquis of Exeter. The former purports to be a portrait of Newton when he was a Bachelor of Arts, and to be painted by Lely. It represents a young man with his hand resting on a globe; and there is an engraving of the picture, done many years back, but he had no hesitation in saying that the picture had no right to be considered a portrait of Newton. The features were

not Newton's, and it was most improbable that the poor Trinity College Sizar would, when a Bachelor of Arts (that is, between January, 1665, and July 7, 1668) have cared, or, if he had cared, could have had the means to obtain a portrait of himself. He apprehended that because the portrait represented a young man with his hand on a globe, some imaginative person had supposed that it must be a representation of the great philosopher who had explained the system of the universe. But this portrait represents one of the poorest specimens of humanity; and certainly not a Newton, but rather a Simple Simon. The Marquis of Exeter's picture has hardly any greater claim to be regarded as a Newton. It represents a man with a bald head, but we have the clearest evidence that Newton was not bald.* The two Vanderbank portraits representing him the year before his death without his wig, show that he had a beautiful head of silver white hair. There is another portrait of Newton by Thornhill, taken at an earlier period, which represents him without his wig, but with short white hair and no trace of baldness. The Portsmouth Kneller of 1689, which represents him when he was 47 years old, shows him without a wig and with abundant grey hair. This is a sufficient ground for rejecting this picture; but no one who has studied the portraits of Newton could be brought to believe that this is a representation of the great philosopher. Last year when reading his paper, Dr. Crompton remarked that he had been unable to discover where the original Knêller of Newton, engraved by Houbraken, was to be found. He then pointed out that in some impressions of the Houbraken print it was stated to be in the possession of Mr. Conduit. Dr. Crompton went to the Earl of Portsmouth's seat at

* Mr. Conduit, Newton's nephew by marriage, when describing Newton's personal appearance, says, "With a fine head of hair as white as silver, without any baldness, and when his peruke was off, was a venerable sight."—Brewster's *Life of Newton*, 1855, vol. ii., p. 413.

Hurstbourne Park, and there found the original picture. It is dated 1702 and signed by Kneller. The Earl of Portsmouth, Newton's collateral descendent, therefore possesses the two most important portraits of Newton. This portrait of Newton was engraved also by Smith, about 1712. A replica, if not an early copy, without any name of an artist upon it, is in possession of the Duke of Devonshire at Holkar. His Grace informs me that he cannot positively trace its coming into the possession of his family, but he conjectures that it may have been at Holker before the marriage of one of the Lowthers with Lady Mary Cavendish, through whom this Holker property passed to the Cavendishes. There is an exact replica or early copy of the Portsmouth Kneller of 1689 at Lord Galway's, except in not having the name of Newton or of Kneller upon it, which Dr. Crompton has examined, and which, in an old catalogue of the pictures, is said to be painted by Bond. Dr. Crompton said that he had examined the three oil paintings of Newton in the Royal Society's collection; also the Kneller at Hampton Court, which is dated 1689, and differs from the Portsmouth Kneller of 1689 only in the position of the hands; also all the portraits of Newton belonging to Trinity College, Cambridge; the Indian ink drawing of Newton in the Pepysian Library at Magdalen College; and a portrait at Mrs. Miles', at Firbeck Hall, Yorkshire, which resembles very much the Egremont Newton by Kneller, engraved as a frontispiece to Sir David Brewster's first Life of Newton, published in the Family Library. Dr. Crompton exhibited this edition of Brewster's life, as well as the larger one in two vols. In the text of both the engravings, which differ very greatly from each other, are said to be from a picture in the possession of Lord Egremont. Sir David Brewster is unable to explain how it is that two engravings from distinct and different portraits should thus happen to be spoken of in the text, except that his publisher must have selected the

second portrait and got it engraved. It is, I think, unmistakeably taken from Smith's print of 1712 or an early impression of Houbraken's engraving. The portrait prefixed to the small Life of Newton is the same as that engraved in Lodge's portraits. At present Dr. Crompton is unacquainted with the historical evidence regarding the genuineness of this portrait. It is signed by Kneller and dated 1716; but Dr. Crompton has not yet seen it, nor one in the possession of Mr. Turner, of Stoke Ashford, near Grantham.

In Lord Portsmouth's collection at Hurstbourne, besides three genuine and authentic portraits of Newton, there is a fourth picture with the name of Newton painted upon it. It was described to Dr. Crompton by a gentleman who had had an opportunity of a very close examination of it, as the earliest and most important portrait of the great philosopher. But Dr. Crompton found neither date nor artist's name upon it, and the picture, which is mounted upon panel, has a crest in red wax behind it, which is not the crest of the Portsmouth family, but could not be sufficiently determined what it was for want of a magnifying glass. The history of this picture will be investigated further; though it is certain that it cannot be a portrait of Newton, for the features are not his, and the eyes are brown, while Newton's were bluish grey. It is most earnestly to be desired that all the portraits of Newton might be collected together at Kensington next year for comparison with each other, and that the portraits of other great men (where there are several) should be thus exhibited in juxtaposition. It would probably then be evident that there exist many spurious ones, and an opportunity would be thus afforded of determining which are the best as well as the true.

Mr. R. D. Darbishire, F.G.S., referred to a paper "On the Existence of a Seabeach on the Limestone Moors near Buxton" (*Trans. Manchester Geol. Soc.*, V., p. 273), in which Mr. John Plant, F.G.S., had described as sea beach the surface of the limestone rock as the same is seen when bared of sward and surface clay above the quarries on Grin Edge and Harper Hill, south-west of Buxton, and to Mr. Plant's conjecture that this worn surface probably extended nearly to the crown of the hills.

His own observation had marked on each hill, above the stratum whose upper surface exhibited those indications of wear, a stratum of somewhat different texture still subsisting in the shape of a slight vertical cliff or reef. This bed had not worn in the same manner as the "beach," that is to say, with many interlacing fissures leaving a close *chevaux de frize* of limestone points, but rather in great blocks with round, curved edges or holes.

In connection with this bed Mr. Darbishire had obtained specimens from each hill exhibiting what he believed to be the remains of the burrows of Pholas shells.

On the top level of Grin Edge, close to the ruins of the tower, one stone had a group of seven holes. They were placed like Pholas holes as he had collected them on Great Orme's Head; and, though the surface of the stone about them was much worn, taken along with the specimen next described, it seemed more fitting to ascribe to them a similar origin than to attribute them to the natural wear of the stone, notwithstanding the variety and singularity of many of the forms in which atmospheric or aqueous corrosion affect the limestone rock.

This stone lay amongst a heap of others near the ruins of the tower, and had doubtless been brought up a few feet. The height above the sea of the tower is stated on the Ordnance Map as 1,435 feet.

On Harper Hill, in two large blocks of the overlying

stratum, he had detected more characteristic holes. Both blocks were lying in the sward above the "beach" surface, and were a few feet below the rock *in situ*, from which they had evidently been detached. Of the first of these specimens he exhibited a photograph. It showed in the under side of the edge of a projecting ledge or table of stone six well marked holes from $\frac{3}{4}$in. to 1in. in diameter, and one an inch deep, and traces of two others. The holes were grouped just as Pholas holes usually are, and apparently were quite independent of the structural fissures of the stone.

According to measurement with an aneroid barometer, the stone in which those holes occurred was about 1,380, and the reef from which it had fallen about 1,400 feet in elevation.

If the holes were really Pholas burrows, they would indicate the elevation of these hills since the period of glacial action by sea or land.

Mr. BINNEY F.R.S., F.G.S., remarked on the great elevation of these remains if the observations were accurate, and observed that they were on the west side of the hills. Mr. Prestwich had discovered shells in shingle on the western slopes of the Axe-edge hills towards Macclesfield, at the height of 1,150 feet. He would like to hear of observations on the eastern side of the Derbyshire hills if any traces of marine action were to be discovered there.

Mr. DARBISHIRE had not the specimens at hand, but would produce them, in connection with a series of similar remains from Orme's Head, to which he proposed to call the attention of the Society at an early meeting.

Ordinary Meeting, October 15th, 1867.

J. P. JOULE, LL.D., F.R.S., &c., Vice-President, in the Chair.

Among the donations announced was a photograph of the memorial recently erected in the Salford Cemetery to the memory of the late Mr. John Parry, presented by Mr. Cottam.

The thanks of the Society were voted to Mr. Cottam for his donation.

"Note on the Occurrence of Sulphocyanide of Ammonium in Gas Mains," by PETER HART, Esq.

A few months ago while some gas mains in the street were being replaced, I was induced to examine the scale which forms in the interior, being under the impression that probably I should find sulphide of iron the result of long-continued action of sulphide of hydrogen on the iron. On placing a portion of this scale in pure hydrochloric acid I perceived an intense reddening, much more than would be accounted for by the simple solution of peroxide of iron, and being aware of the fact of sulphocyanogen being one of the products of the distillation of coal, I at once suspected its presence. A portion of these scales were boiled in water. The clear filtrate from this gave off much ammonia on the addition of alkali, and on the addition of a dilute solution of perchloride of iron it gave at once the intense coloration so characteristic of the sulphocyanides. The insoluble portion remaining on the filter was then boiled in dilute caustic

soda; the filtrate from this made acid, and a solution of ferric oxide again added, this time with the production of a blue precipitate indicative of a ferrocyanide. This must have existed as ferrocyanide of iron, which on boiling with the alkali became oxide of iron and ferrocyanide of sodium. There appears to me something curious in the fact of these bodies being carried such a distance (in this case fully a mile from the gas works) by the gaseous current. I should think the ferrocyanogen is the result of a reaction between the sulphocyanogen and the metallic or oxide of iron. The amount of these bodies must, I think, be far too small to have any bad effect on the health of gas consumers.

"Jupiter as observed at Ardwick on the night of August 21st, 1867," by J. B. DANCER, F.R.A.S.

The somewhat rare phenomenon of Jupiter without a visible satellite extraneous to his disc (as the Rev. W. R. Dawes has correctly designated it) has been described in the *Astronomical Register*. I am not aware, however, that any notice of it has appeared before this Society, and as few observers appear to have enjoyed such favourable atmospherical conditions as we did in this locality, I am induced to offer a few remarks on the appearance which Jupiter presented on the night of August the 21st.

During the early part of the evening the S. and S.E. portions of the heavens were covered with thick haze; between 8 and 9 o'clock this gradually disappeared, and the atmosphere became clear and unusually favourable for astronomical observations. Preparations had been made for noting the times of the phenomena, but from various circumstances this was abandoned. The observations were made with an achromatic telescope of $7\frac{1}{2}$ feet focus and $6\frac{1}{4}$ inches clear aperture. Powers employed, 90, 175, and 300. At 9 o'clock Jupiter was clearly defined and presented a

remarkable appearance. On the broad south belt three spots were distinctly visible, and at the same time three satellites appeared off the disc. The first spot in the order of transit was very black—this was the shadow of the third satellite. The next spot was brown in colour when contrasted with the belt—this was the third satellite; and the third spot, which was the shadow of the fourth satellite, was very dark and well defined. At this time the fourth satellite appeared very faint in comparison with the first satellite, which was just below it. The second satellite, which was on the west edge of the planet, was soon eclipsed, and attention was entirely directed to the fourth satellite, which was the next to transit. As this satellite entered on the disc it was visibly brighter than the planet; after a short time it disappeared, and then, to my surprise, it became visible again as a dark spot, and at intervals it was as black as its own shadow. The alteration in the colour of this satellite during its transit has been noticed by several observers, but I was not prepared for such a complete change from a bright object to one as black almost at times as an ink spot. During these observations the first satellite had entered on the disc of the planet, and when it was about half its diameter on the planet it appeared to me to be in contact with its own shadow. After a short time I could only distinguish the first satellite at intervals, closely following its own shadow. Before the third satellite passed off the disc it became nearly invisible, and when close to the edge of the disc it appeared brighter than the surface of the planet. The fourth satellite was not observed at the end of its transit.

Mr. Robert Worthington, F.R.A.S., observed some of the phenomena with me, and remarked at the time that he did not recollect having seen Jupiter so sharply defined as on that evening. The bays and various markings on the belts were most beautifully distinct.

"Notes on some Superficial Deposits at Great Orme's Head, and the Period of its Elevation," by R. D. DARBISHIRE, B.A., F.G.S.

After referring to Professor Ramsay's exposition of successive glacial periods in North Wales and its elevation; (*Quarterly Journal Geol. Soc.* viii., and *Old Glaciers of Switzerland and N. Wales*, 1860) to a paper by Mr. Binney, F.R.S., on "the Drift Deposits found about Llandudno" (*Geol. Soc. Trans. Manchester*, April, 1861), and a paper by Mr. Bonney "On Traces of Glacial Action near Llandudno" *Geol. Magazine* iv., July, 1867), the author shortly described the configuration of the Head and the Llandudno Isthmus. He supposed the superficial outline of the Head to be due to marine denudation, possibly aided at some early period by the friction of floating icebergs, but in the last instance and ever since its first emergence to long continued and extremely tranquil elevation and concurrent sæcular subaerial degradation and deposit. The isthmus, he described as ancient sea bottom, gradually elevated and denuded in the process by current or tide wash, and then augmented by beach accumulation and blown sand.

Detailed notes as to the superficial formations were then read as follows:

I.—MARINE FORMATIONS.

(1) Yellow cherty clay in Gwydfyd Valley, as described by Mr. Maw, F.G.S., *Geol. Mag.* ii., 1861.

(2) Glacial and boulder clays of a later age:

(*a*) Tenacious dark blue clay, full of small pebbles of dark slaty rock, at the foot of the western cliff of the isthmus and on the beach, as described by Mr. Bonney, *Geol. Mag.* iv., p. 292.

(*b*) Close greyish olive coloured rather sandy clay, with rounded stones, in similar situations (possibly a variation of (*a*.)

(*c*) Chocolate brown clay, with boulders, some scratched and occasionally slightly bedded, forming the general base of both eastern and western cliffs of the isthmus. Formerly well exposed where the new sea wall and boatstairs are near Tygwyn, Llandudno. At this spot fragments occurred of Tellina solidula, Mactra solida, Astarte arctica, Cardium edule.

The same bed appears at Colwyn ballast cutting under sandy clay and old beach shingle, with fragments of the same species and of Saxicava.

(*d*) A very similar bed of lighter coloured reddish clay with rounded and angular stones, in the west cliff, and generally over the isthmus.

In the cliffs occurred fragments of Mya truncatia, Tellina solidula, Mactra ——, Cardium edule, Mytilus edulis, and Buccinum undatum.

The same bed and like fragments of shells are seen at 70—80 feet above the sea near the bath house, and according to Mr. Maw, as high as 170 feet up the Gwydfyd Valley.

Mr. Binney had found Turritella terebra, and had pointed out the similarity of those beds with those at Blackpool. Mr. Maw had identified the boulder clay also in a terrace about 170 feet high on the south.

The occurrence of travelled boulders indicated the former existence of the bed at higher levels; thus a mass of greenstone had occurred above Gwydfyd farm at 380 feet elevation.

These boulder clays, the author supposes to have been deposited at the close of the glacial epoch, and to be the result of redistribution of older beds on the coast of a rising land, mixed with old drift and land drift, swept down by the glaciers of Snowdonia, in whose recesses the astarte and other shells occur on ancient beaches.

I. B.—Sea Beach and Beach Marks.

(3) Mr. Binney had described a bed of shingle at 400 feet in the valley of the old road.

(4) Mr. Bonney had described as an indication that the surface of the higher ground has in many cases been affected by ice, large blocks of limestone scattered about, especially on the north-west of the hill, differing from the rock on which they lie, but of a stone found *in situ* on a slight eminence in the neighbourhood,

The author believed these rocks to have no glacial origin, but to be raised beach—the remains of the rage or play of waves about the new-born island.

He had not found any boulder of other stone than that of the hill, and could not indicate any other origin for these elsewhere. Puffin Island and Anglesey to the west are much lower, and Penmaen Rhos to the east lies out of any hitherto described extension of the North Wales glacier system.

(5) The plateaus furnished other indications of sea-wear, as on many of them the limestone stratum is uniformly denuded of all superincumbent stone, and cut and worn in fissures and other forms very similar to those of like beds between tides below. Whether this erosion is that of ancient waves, or of rain water, since elevation the even stripping of the overlying stratum indicates aqueous denudation.

A similar or even more marine looking scar forms the top of Little Orme's Head.

The vertical outlines consisting, when not hidden by talus, of alternate cliff and scar, and the grand curves of successive contour lines of the Head are thoroughly marine.

Mr. Bonney had noticed the hollows of wave wear in the face of these cliffs.

These longitudinal caves record a long continuance of tide wash on a stationary cliff. The undisturbed horizontality of many of these wave marks indicates great uniformity in the process of elevation.

(6) Distinct traces of raised beach occurred on the east side of the Head near the bath house, where masses of shingle hang cemented to the face of the rock 50—60 feet above the sea.

(7) On the boulder clay of the isthmus, especially developed in the western cliff, is a deposit of varying thickness of beach shingle, courser or finer; seen also in foundations in Llandudno, and probably continuous with the present eastern beach.

(8) On the last named beds lies much blown sand. Mr. Glover mentions that within forty or fifty years this sand has converted into solid ground a tract of impassable swamp, and even overlaid it.

(9) Unmistakeable proofs of beach wear occurred in the shape of the burrows of Pholades, necessarily made while the rock was near if not below low water mark.

These remains occur at various heights all over the Head, under certain conditions, of sufficient consistency of stone and sufficient shelter from atmospheric wear, either under the edges of tables of rock, *in situ* or fallen, or in rounded beach stones, on or in the sward on the talus slopes of the hill. All the stone in which holes had been observed, had evidently suffered some superficial waste, hence only the ends of deeper burrows remain.

The following list is arranged along a rude profile of the Head, from N.E. to S.W.

(*1*) Two slabs near the stile on the path from the archery ground to Gwydfyd farm. 216 feet.

(*2*) In the cutting over the yellow clay at Gwydfyd farm in a large loose boulder, three large and very perfect burrows and part of a fourth, possibly of pholas crispata, a species of well ascertained quaternary occurrence. 350—60 feet.*

The same spot furnished a second boulder with ten holes.*

(*3*) The rugged hillside above, and to the west of the clay pit, yielded in the edges of beds *in situ*, well preserved groups of holes at 370 feet and 435 feet, above the sea.

(*4*) The first rock that breaks the sward on the south side of the path, up to the old telegraph station, after leaving the mine plateau, showed good holes, not to be confounded with the effect of atmospheric corrosion. 440—450 feet.*

(*5*) In a reef which breaks the sward in the plateau S.W. of the telegraph hill, with worn beach stones, occurred a fragment with two holes. 570 feet.*

(*6*) On the exposed reefs towards the south, the holes are not unfrequent. On the 2nd or 3rd reef, marked by a loose spherical boulder at its eastern end, a fallen slab of very hard stone, showed 27 holes. 570 feet.*

(*7*) The next reef yielded good specimens at 540 feet,* and at 520 feet.*

(*8*) The burrows occur in considerable abundance and good preservation, at Little Orme's Head, especially on the topmost plateau.

* In these cases specimens were produced.

In the preservation of these holes in loose beach stones, and in the edges of out-cropping layers of stone, in the level sea stripped scars, and in the limestone boulders of the higher plains, the author submitted irrefragable proof that the surface of these anciently submarine hills had not been touched by iceberg or glacier for some time before or at any time after they emerged from the waves.

He had not been successful in characterising any rock surface on the Head pointing as montonné, nor in finding the rock which appeared to Mr. Binney, to have "been scored and polished probably by ice," below the bath house.

I.—C. Old Land.

(*9*) Between tide marks at the western foot of the Head some years ago, after the removal of shingle, a light blue clay without stones was seen, and traces of a vegetable deposit of leaves. At Abergele and Colwyn to the east, and under Penmaenbach to the west this vegetable bed occurs with roots and stems of forest trees. From a cutting near Penmaenbach, where a part of the bed appears as peat of very modern aspect, had been obtained bones and a tooth of an ox, and a large horn of red deer. A similar horn was found near Abergele when the railway works were in progress.

II.—Subaerial Deposits.

(*10*) On each side of the Head there appears on the rock unstratified reddish calcareous clay with more or less frequent, larger or smaller, angular fragments of the overhanging rocks, not seldom so numerous as to form a breccia. Here and there, at considerable elevations both north and south of the hill, this bed yields rounded and larger or smaller pebbles of greenstone, and other non-calcareous rocks; and not unfrequently pieces of limestone with the softly curved surfaces of sea wear.

(*11*) On most of the less elevated or less steep inclines, the last mentioned deposit is overlaid by, and passes upwards into a very similar layer, distinguishable by a darker colour, and more earthy consistency, and containing like angular sea and worn fragments. In this bed, the flatter fragments of stone are noticeably disposed in planes parallel to the slope of the sward. It is this last-named bed which Mr. Binney traced as blackish brown clay, with angular fragments up to 262 feet above the sea.

The position of these two beds of clay at the foot of sea worn cliffs, the almost total absence of rolled stones, the entire want of tidal or current assortment of material, and on the other hand the predominance of sharp angular stones, their promiscuous aggregation, and the slight bedding along the surface line mark them as not marine and as subaerial. The upper formation is much modified by the gradient of slope and the luxuriance of vegetation.

The clayey matrix probably arises from decomposition of the rock fragments; but the occasional occurrence of rounded pebbles of greenstone, &c., may indicate a redistribution of patches at least of boulder clay.

It is in these two beds, and especially in the upper one, that observers have found shells of mussels, oysters, limpets, and periwinkles. Mr. Dancer apparently was the first to mention these remains in Manchester, and Mr. Sidebotham (from whom Mr. D. received his first specimens), shortly after exhibited large collections. In 1861, Mr. Binney mentions having tracked these shells up the hill towards a bed of shingle at a height of 400 feet, and deduced proof of the elevation of this Head at least that height during a very modern epoch.

In 1867, Mr. Bonney (*Geol. Mag.* iv. 289) noted a deposit of shells near Gwydfyd farm, in clay which appeared to him "not to have undergone much denudation during the process of upheaval." Mr. Maw had stated, *Geol. Mag.* iv. 377, his conviction that Mr. Bonney's shells had been deposited by man. Mr. Darbishire pronounced the shells of the red clay with angular fragments also to be of non-marine deposit. These latter shells are of the species Mytilus edulis, Cardium edule, Ostrea edulis, Patella vulgata, Littorina littorea, Purpura Capillus, and Buccinum undatum. These occur either massed in heaps or layers (generally of one or two kinds together) or more sparsely distributed. They are all of species eaten by birds or man. All the specimens are full grown, or at least of edible size, and in nearly every instance whole, the exceptions being Mytilus, a shell peculiarly liable to disintegration, and the univalves, which sometimes occur broken about the mouth, as if to allow of extraction of the animal. There are neither any other species nor young specimens of what do occur. The shells do not appear as if deposited under any natural original conditions, nor, if they were so, does it seem likely that they would have remained during a slow elevation through the tides of ages. The fact of elevation is undoubted, but these shells are no evidence of it.

It is not necessary to give separate lists of the lots found at successive heights. They occur on all sides of the Head, but have been especially collected in the old road valley, and on the Hendafarn road towards the south-east. Along this district they have been noted at 120, 140, 180, 200, 300, and 315 feet above the sea. On the old road section, Mr. Darbishire had found the shells

not unfrequently accompanied by teeth, and fragments of bone of domestic animals, and bits of charcoal. Thus there occurred:

At 120 feet, part of the jaw of a pig.

At 180 feet, sheep's teeth and frequent fragments of bone.

At 300 feet, the molar of a horse and fragments of bone.

At 315 feet, on the north side of the road were found with patella and littorina, a cow's tooth, part of the jaw, and other bones, and teeth of a pig, fragments of other bones, bits of charcoal, and two human molars.

At 365 feet, fragments of bone.

The bones have all lost much animal matter.

On the whole these shells and bones must be attributed to deposits made by birds or men. The shells are such as sea birds often carry to land for food, and (except the oysters), most of them may now be found in suitable places on the Head so transported.

In Connemara shells of the very same kinds may be frequently found and traced to or from a cabin or the site of one, on or under the sward according to the overlying of the deposit.

Shells lying on exposed ledges, or shells or bones cast out, would in the course of years be distributed down hill along with and in time be buried in and under the superficial detritus. * Wind and rain and chance footsteps would assist in scattering such objects.

It is probable that the deposit of these remains on the Head is of very ancient date.

* At the Tunara sand flats, north of Gibraltar, I have watched a levanter roll along heavy shells of Cardium tuberculatum, and Pectunculus, and even a massive valve of Panopaea glycimeris.—R. D. D.

III.—Refuse Heaps and other undisturbed Relics of Human Occupations.

(*12*) Mr. Bonney's bed of shells at Gwydfyd pit has been mentioned. He had collected Mytilus Ostrea, Patella, and Littorina with three valves of a Tapes doubtfully identified as pullastra.

The deposit was entirely within the uppermost layer of blackish earthy clay. Mr. Darbishire's collections from this spot comprised Tapes decussatus (but no pullastra), (in pairs and valves, whole and broken, all full grown and some large), Cardium edule, Mytilus edulis, Ostrea edulis, Patella vulgata, Littornia littorea, Littorina littoralis, Purpura lapillus, and Buccinum undatum; all species still eaten. No bones or implements occurred. The deposit is now removed by the enlargement of the clay pit. It is undoubtedly human, and probably (from its depth), older than the next deposit.

(*13*) A very extensive midden or collection of such deposits has been exposed in the slope of talus below the western inland cliff, where it is itself cut by the waves into a low shore cliff, north of the site of what is now the house of the Dean of Christ Church. It has long been open on the cliff section, and for some years also on the slope in a cutting there, to an extent of over 30 yards up the incline, and 150 at least along it.

Mr. Bonney has published full observations on the cliff sections. *Geol. Mag.* iv. 343.

The remains consist of layers or heaps of Mytilus edulis, Patella vulgata, Littorina littorea, with some oyster shells and Purpura lapillus, and at least one heap of Buccinum undatum. At the lower edge the heaps lie massed. Higher up the process

of distribution has begun. The whole is grass covered.

Bones, teeth of ox, sheep, and roebuck, and of some birds occur. Burned bones not unfrequently, and once a small bit of red earthenware occurred, but as yet no implements.

Nothing appears in the situation or condition of these remains which indicates great antiquity.

(*14*) Lastly, Mr. Bonney mentions as occurring at intervals in the sand cliffs on the Conway Bay shore seams of Mytilus edulis, which he assumes to indicate "a period of depression, during which the mussel beds are formed," and of subsequent upheaval.

These must be set down as appertaining to the most recent human period.

The practice of dredging great quantities of mussels in the estuary, of boiling them on the shore, in order to search the animal parts for pearls, is of old standing, and has been frequently described. See *Loudon's Magazine of Nat. Hist.*, 1830, as to the mussel beds and heaps on the western shore. Mr. Thomas Glover has often seen them in operation on the eastern side of the bay, *i. e.*, making these beds.

Mr. D. concluded with a notice of Mr. Binney's bed of shingle at 400 feet elevation, which called for special consideration, and postponed these remarks for discussion on a future occasion.

Mr. Sidebotham's accidental absence was much regretted, as that gentleman had paid much attention to the deposits on the Head. A full series of specimens of pholas holes from Great Orme's Head and Little Orme's Head was exhibited and examined, as were also specimens from Grin Edge and

Harper Hill, Buxton, referred to by Mr. Darbishire at the last meeting. The character of the holes as true pholas burrows, was in each case fully recognised.

Mr. BOTTOMLEY remarked that the specimens of limestones appeared to be decidedly argillaceous, which would tend to make them less liable to atmospheric decay.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 7th, 1867.

J. B. DANCER, F.R.A.S., President of the Section, in the Chair.

The PRESIDENT in his address to the members of the section, described the various additions and improvements which had taken place in microscopes and apparatus, and gave a summary of the Microscopical researches which had been communicated to various societies, both English and Foreign, during the past year.

The following extract of a letter dated 27th of August, 1867, from Captain Mitchell of Madras, was read:—

One or two things have come under my notice, which a friend who arrived in Madras from London a short time since, said, he believed were unknown to microscopists in England, I therefore send you a brief notice of them, in the hope they may interest some of the members of the society.

The first is the presence of ciliated infusoria in dewdrops on leaves. I have to confess I took this subject up in jest, in consequence of a remark in an article published in one of our local papers; about sunrise I placed an animalcule cage under the point of a leaf and transferred the drop of dew gathered there to the glass plate, and then examined it with the microscope. I repeated the experiment on two

other occasions, and the result was, that in about one drop out of every two I found one, and sometimes two infusoria.

As the dew did not begin to fall until midnight, they must have been produced from the germ between that time and 6 a.m.

On the first occasion I saw also what I took to be one or two spores of fungi. I supplied the cage with distilled water and put it by until the next morning, when I found a perfect forest which continued to multiply so long as I supplied it with distilled water, which I did for several days. It will be interesting to ascertain if infusoria are found under similiar conditions in England.

Some time ago Mr. Ross sent me a binocular body for my stand (one of his father's make). I have in my cabinet the tongues (so called) of two house flies, which I had mounted some years back. I was always under the impression that the divided absorbent tubes were enclosed between the two membranes that form the upper and lower surface of the two lobes, and I believe that is the general opinion of their structure; now on placing one of these specimens under a half-inch glass with the binocular body, I was not a little astonished to see the tubes standing out above the surface of the membrane on the lower or under part of the tongue.

I was always at a loss to understand the use of these very curious vessels, but it now seems evident that fluids may be taken up by them and conveyed to the larger central tube into which they all run. It is I presume known that these tubes all open on the upper surface of the lobes by the other and narrower extremity.

Mr. LATHAM read the following communication on Silk-producing worms from Natal :—

In the *Natal Herald* of the 8th August last, there are copies of a correspondence between the Chamber of Commerce of this place, and some gentlemen at Natal, regarding

certain silk-producing worms, as they are termed, found near Graham's Town by Mr. Hillier, feeding on the leaves of the mimosa thorn or acacia.

The result of the correspondence was, that some of the cocoons were presented to the Chamber here to have them reported on and their true value ascertained.

They came into my hands, and the following remarks on them may be interesting to the Society:—

From one of the cocoons the moth now exhibited emerged shortly after its arrival in England, and though much crippled, I have, through the kindness of Mr. Jansen, had it clearly identified with the insect "Pachypasa effusa," of which there are several specimens in the British Museum collection from Natal.

The moth laid about 50 eggs, of which I have mounted two or three and they are here for examination.

The eggs under the microscope exactly resemble in texture those of the ostrich, but each has a small black point, probably of a softer substance than the rest of the egg, and through which the caterpillar may emerge.

From one of the cocoons I extracted the chrysalis also exhibited, and further the cast skin of the caterpillar rolled into a small ball as usual; by boiling this for some time in caustic potass, it became so softened that it was possible to get it to its original size, and to show its original form. You have it before you dried, and the series is therefore complete, egg, caterpillar, chrysalis, moth, and cocoon.

The original cocoon is, you will notice, a hard woody-like substance, but by certain processes, Mr. Hillier states soaking in a solution of soda, the cement agglutinating the silk is dissolved and a soft silky-looking bag remains.

This consists of a thick outer covering, a loose middle lining, and a thinner internal lining, all of silk, which I hardly think could be wound but might possibly be carded.

As regards the commercial value of the article, an eminent silk broker writes as follows :—

"It is a carded cocoon, the waste silk is the outer covering of the cocoon and appears of tolerable fine fibre, but is bad in color and not of a good merchantable appearance, if in quantity worth perhaps 1s. 6d. per lb. all round."

"Enquiries of a similar nature have been made from time to time by customers in the East, where silk does not form one of the staples of the country, and among other places, from Natal, but we do not find that any good can arise from efforts made to produce silk in any quantity; it is probably the most difficult of all known staples to establish in a new land."

Ordinary Meeting, November 12th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. John Barrow was elected an Ordinary Member of the Society.

Mr. BINNEY, F.R.S., F.G.S., said that in the Rev. J. G. Cumming's excellent History of the Isle of Man that author, at page 132, says, "the different layers of the Posidonian Schist vary both in their lithological texture and organic contents. The finest and most compact layer, which is worked for ornamental purposes, is characterised by an abundance of Posidonia and the relicts of tree ferns, which we must necessarily regard with interest as indicating an approach, though still at a considerable distance from the coal formation of Great Britain." As the discovery of fossil tree ferns in the mountain limestone would be of great interest, he had lately been over to Poolvash Bay, the locality named by Mr. Cumming, and spent a considerable time in searching the black limestones there for those fossils; but although he met with plants, the remains of vegetables common to the carboniferous formation, such as *Stigmaria ficoides, Calamites,* and other coal plants, he found nothing resembling tree ferns. The state in which the roots (*Stigmariæ)* were found in this limestone led him to believe that Mr. Cumming had mistaken them for tree ferns. The depressed areolæ in this fossil have the lower portion of the radicle attached to them so as to give the appearance of a scar not much unlike that of a tree fern, but he convinced himself that they were unquestionably *Stigmaria.*

This portion of *Sigillaria* is often met with in the beds of limestone in the Yoredale series of Professor Phillips, as well as much lower down in the carboniferous series in the limestone of West Calder, near Bathgate, Scotland, where it occurs in great abundance. He also stated that the lower coal seams of Scotland containing workable beds were of considerably earlier date than the Posidonian Schists of the Isle of Man.

Dr. R. Angus Smith, F.R.S., said that he had frequently visited some parts of the Continent, and had been a student of exhibitions such as included the useful arts. He was quite prepared to agree with those who saw a very great improvement in the touch of the workman, in the countries nearest to us in the north of Europe especially. In France the advance made was very great, and so also in Germany; but in neither of these cases did he consider that the exhibition showed the true state of the arts among the people. A true exhibition would give the proportion of bad and of good manufactures. There was no attempt to do this except in cases where the objects were viewed rather as curiosities, as for example, when costumes and architecture were introduced. When a number of knife makers showed their manufactures, it was clear that they all showed their best, but they did not show how many made knives that would scarcely cut, and even if they had done so, it would not have been sufficient to exhibit the state of the arts among the people. It would not inform us how many of the population had no knives set down to them at dinner, and how many houses had not a fork of any kind. In one of the countries which had made greatest progress and showed beautiful work of all kinds at the exhibition, he had been in an hotel where no fork whatever existed, and when he asked for a knife, the landlady handed him one from her pocket. Yet she and the landlord were very respectable

looking people, and the house was clean. True, it was only in a village, but it was only a few miles from a considerable town. The people were not the poorest, but probably the richest in the village. He had not found any similar case in Great Britain. These were decent people of the lower classes in want of what we may consider the ordinary tools requisite in modern civilisation only three years ago. He did not wish to speak of any manufacture for the study of which he may have had special opportunity, but, speaking generally, he did not think that the modern changes had penetrated all classes of the community so deeply in France and Germany as the exhibition represented, whereas England had done more than it showed at the Paris exhibition. This is quite independent of the question which is best, and relates only to the useful arts.

It is clear, however, that wonderful advances have been made, and who makes them? He considered that they were made by the upper classes of the manufacturers, and used by the upper and middle classes, but had not descended universally even to the middle classes. When the general descent takes place, the manufacturers will have a much larger home market than they have at present. It is for the interest of England that this advance among the people should take place, as in some instances it will prevent competition in foreign markets; but whatever the commercial result may be, there is one lesson which we may all learn. Within the last thirty or forty years, the violent attempts to teach the people here by schools, mechanics' institutes, and lectures given or promoted by benevolent persons attaching themselves to various societies, have wearied the souls of all who have cooperated or even looked on with interest. In Germany, without any commotion, calmly and pleasantly, the youths have been trained in schools and colleges without number, and so thoroughly that they are able to supply foremen and managers to their own manufacturers'

establishments, and to send a supply also to foreign countries, without diminishing the supply of that higher order of men of learning that have so long made Germany famous. In other words, whilst we have failed with the most violent efforts and with much noise to teach our own, they have succeeded not only to teach their own citizens, but to assist in educating the rest of the world. In this matter of education, the Governments have been able to mould the nation's mind, and to alter the habits of a leading portion of it in a very few years. A careful education would probably show its influence in less than ten years. True, many questions arise; and we may ask if education would not smooth down the peculiarities of the national character, and prevent it trusting to individual will and genius. Pedantic education might do this, but training is certainly as requisite in civil as in military affairs, and no one says that discipline diminishes the power of an army. It may be a question whether the uniform organisation of education in France is not calculated to produce too much equality in the minds of the nation, but it will certainly produce immense power in the aggregate. The numerous small states of Germany, each fostering its own schools and universities, seem best fitted to produce an intellectual activity. We see there, many universities, each developing its own peculiarities. They must make the nation more many-sided, and they have done so. At any rate, the present lesson seemed to be that the more intelligent part of a nation may be fashioned anew in a few years by its instructors as easily as a boy may be taught to be either a shoemaker or a tailor; cases of original bent excepted. We desire schools in this country, but cannot find them; there is no organisation for making them to the extent they are required. The genius of this nation, great as it really is, impatient of details, and looking vigorously to results, will never compete in a wild state with the disciplined army of thinkers and of

manufacturers abroad. Those who do succeed here must train themselves, and to that training they owe their success, but it is hard work, and not to be expected of many. However, it is clear that an entirely new spirit may be made to animate society in the course of a single generation, and a nation may be born in our own day; also, as the result has been, so far as the useful arts are concerned, exactly that which the rulers desired, we must believe that governments or large combinations of men have the change in their power.

It was said that this progress in the arts did not necessarily go down to the whole of society. There may be several reasons for this: the upper classes may be far advanced and the lower very poor, even when there is much good feeling. In some of the mining districts of Germany, sending out the most intelligent miners to distant parts for centuries, and teaching their own with great care in schools which have been imitated, but not surpassed, we find a population extremely poor. Nature has presented little to them. We cannot expect all farmers to be equally rich, when soils differ so much. In such cases, however, we can expect a careful superintendence and a thoughtful mode of husbanding resources, mitigating the evils of poverty, and producing content where otherwise abject misery would exist, and that we find.

The exhibition shows how much may be done for the active minds of nations by a government fostering education, and the state of the same countries shows that intelligence, comfort, and wealth have been promoted also. Whilst the poorer parts alluded to, in Saxony for example, show that when from natural causes wealth has not been accumulated, education has produced intelligence to mitigate those evils which would otherwise have crushed the people. This education is owing to the activity of the governments. We learnt the lesson in this island once, and forgot it. We

must be humble enough to learn it again from the world, instead of teaching it to them as we ought to have done.

The unassisted working classes in this country have raised themselves to an enjoyment of the products of civilisation not equalled probably anywhere in Europe, and their progress has been of longer duration; as we love our country we are still disposed to believe in its power of keeping in advance. But how could a regiment, however brave, advance with longbows against modern artillery ?

We require education in the fundamental principles of physical science; the moral principles as found in literature are not, when alone, sufficient either for the higher cultivation of the mind or every pursuit of the useful arts. This applies to the rich and not merely to the poor.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 4th, 1867.

J. SIDEBOTHAM, Esq., in the Chair.

The Rev. J. E. Vize showed and presented to the section four specimens of insects beautifully mounted in balsam by himself.

Mr. SIDEBOTHAM read the following "Note on the Ship-barnacle."

On the 28th of September I was at Lytham with my family. The day was very stormy, and the previous night there had been a strong south-west wind, and evidences of a very stormy sea outside the banks. Two of my children came running to tell me of a very strange creature that had been washed up on the shore. They had seen it from the pier and pointed it out to a sailor, thinking it was a large dog with long hair. On reaching the shore I found a fine mass of barnacles, *Pentalasinus anatifera,* attached to some staves of a cask, the whole being between four and five feet long. Several sailors had secured the prize, and were getting it on a truck to carry it away. The appearance was most remarkable, the hundreds of long tubes with their curious shells looking like what one could fancy the fabled Gorgon's head with its snaky locks.

The curiosity was carried to a yard where it was to be exhibited, and the bellman went round to announce it under the name of the sea lioness, or the great sea serpent.

I arranged with the proprietor for a private view, took my camera and a collodio-albumen plate, and obtained the photograph I now exhibit. The afternoon was very dull, and the plate would have done with a little longer exposure, but this along with the specimens I show, will give some idea of the strange appearance of this mass of creatures.

This barnacle is of interest as being the one figured by Gerard as the young of the barnacle goose. As some of our members may not have seen the book and read the quaint description, I have brought my copy of Gerard's Herbal for their amusement.

I may just mention that another mass of barnacles was washed up at Lytham, and also one at Blackpool, the same day or the day following. I did not see either, but, from description, neither was so fine as the one I have described.

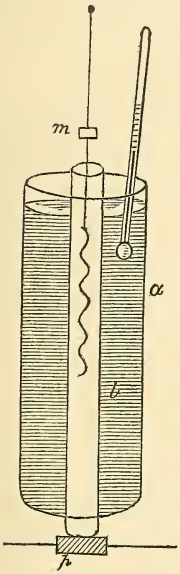
This mass of barnacles was evidently just such a one as that seen by Gerard at the Pile of Foulders. It is rare to have such a specimen on our coasts. The sailors at Lytham had never seen anything like it, although some of them were old men who had spent all their lives on the coast.

Ordinary Meeting, November 26th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. James Stanley Kipping was elected an Ordinary Member of the Society.

"On a Thermometer unaffected by Radiation," by Dr. J. P. JOULE, F.R.S., &c.



In the annexed figure a is a copper tube about one foot long, and has a tube open at both ends in the centre. Water is poured into the space between the two tubes. In the centre tube there is a spiral of fine wire suspended by a filament of silk, and having a mirror at m. There is a lid at p which can be removed at pleasure from the lower end of the tube. When p is situated as in the figure, there can be no draught, and consequently the spiral with its mirror is at zero of the scale. But when p is removed, there is a current of air which turns the spiral, if the air in the tube has a different temperature from that of the outside atmosphere. In my apparatus, one degree Fah. produces an entire twist of the filament. I find that the temperature in the tube is generally warmer than in the outside atmosphere of a room, which must be owing to the conversion of light and other radiations into heat on coming into contact with the copper tube. I have tried the apparatus in the open air on a still day, with the same result. Of course

when there is wind the effect is masked, but I feel confident that by increasing the length of the tube, making it 30 feet for instance, and using certain precautions, this difficulty may be overcome.

"On Solar Radiation," by JOSEPH BAXENDELL, F.R.A.S.

Although observations of solar radiation have now been regularly made for several years at various public observatories, and by many amateur meteorologists, I am not aware that any useful or important result has yet been deduced from them. It seems to be generally supposed that the disturbing influences which affect the indications of the black-bulb thermometer are so uncertain and irregular in their action as to render it almost hopeless to expect that any new and valuable result can be obtained from them. On comparing sets of observations made by different observers, the most startling and discouraging discrepancies are often found to exist, for which, in the absence of any information as to the exact circumstances under which the observations were made, it is impossible to account satisfactorily. A few years ago an enquiry in which I was engaged, led me to undertake a discussion of the Greenwich solar radiation observations; but the results proved so perplexingly anomalous and unsatisfactory, that I could not venture to place any reliance upon them. Having, however, recently become possessed, through the kindness of the Rev. Robert Main, F.R.S., and the Trustees of the Radcliffe Observatory, Oxford, of copies of the volumes of Radcliffe Observations for the years 1858 to 1864, and finding that they contained a valuable series of solar radiation observations, I have been led to resume the subject, and although the enquiry is still incomplete, I have thought that some of the results already obtained are sufficiently curious and remarkable to render it desirable to bring them before the society, in order that attention may be drawn to a much neglected

but highly interesting branch of enquiry, and to the necessity of devising and adopting a more reliable and systematic method of determining the intensity of solar radiation than the one at present in use. It is much to be regretted that no regular and long-continued series of observations has ever been made with Sir John Herschel's actinometer, since there can hardly be any doubt that such a series would yield much more accurate and reliable results than can be obtained by the use of either the ordinary or the *vacuo* black-bulb thermometer.

The Oxford solar radiation observations commenced on the 2nd of January, 1856, and were made with a thermometer supplied by Negretti and Zambra. On the 3rd of November, 1858, this thermometer was unfortunately broken, but a new thermometer being obtained, the observations were resumed on the 1st of December, 1858, and continued uninterruptedly till the 11th of September, 1864, when the second thermometer was also broken; a third thermometer was obtained and brought into use on the 17th of September, and the observations continued with it to the end of the year. The entire series of observations during the nine years 1856–1864, must therefore be regarded as consisting of two distinct series, made with different thermometers, and therefore not strictly comparable with each other. I have, in consequence, thought it desirable to confine the discussion, in the first instance, to the observations made with the second thermometer during the years 1859–1864, more especially as in 1857 the readings of the first thermometer were only to the nearest whole degree, and in 1858 the month of November was omitted; and in order to make six complete years, I have included the observations made with the third thermometer from the 17th of September to the end of 1864.

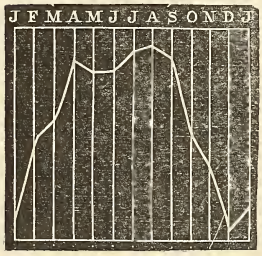
The following table shows the mean monthly and annual differences between the maximum temperature in the shade

and that in the sun during the six years 1859–1864, and the monthly and annual means of the entire series:—

	1859	1860	1861	1862	1863	1864	Monthly Means
	○	○	○	○	○	○	☽
January...	7·3	5·7	5·7	4·2	4·9	5·3	5·51
February..	10·9	11·5	9·0	6·8	9·2	7.6	9·16
March ...	11·7	11·2	10·4	7·6	10·2	9·4	10·08
April	13·2	16·1	15·4	11·6	11·8	11·3	13·23
May	15·0	13·5	13·5	11·6	11·5	12·3	12·90
June	16·0	12·3	14·4	10·1	13·2	11·6	12·93
July	17·8	12·8	12·4	13·4	14·2	12·3	13·81
August ...	16·3	12·5	15·1	13·3	12·5	14·5	14·03
September	16·3	11·8	14·1	12·5	12·8	14·8	13·71
October...	11·1	8·7	11·0	8·8	8·4	10·0	9·66
November	12·4	8·4	7·9	8·2	5·4	6·5	8·00
December.	6·2	4·9	6·0	4·4	3·3	2·2	4·50
Annual Means	12·85	10·78	11·24	9·37	9·78	9·81	

A projection of the mean monthly values gives the curve in diagram A, from which it will be seen that the maximum amount of radiation occurs in August, or about a month later than the time of maximum temperature; whilst the minimum occurs in December, about a month earlier than the time of minimum temperature. There is also a slight secondary maximum in April. This curve, therefore, differs from that of any other element of temperature; but observing that the time of minimum corresponded exactly with that of maximum in the curve laid down from the numbers given in a table which I communicated to the Physical section on the 5th of March, 1863, showing the monthly sums of the oscillations of mean daily temperature at Greenwich during the thirteen years 1848-60, and also the mean daily values for the different months; and that the time of maximum agreed nearly with that of minimum disturbance of mean daily temperature, it occurred to me that the two phenomena might be closely connected with and dependent upon each

A

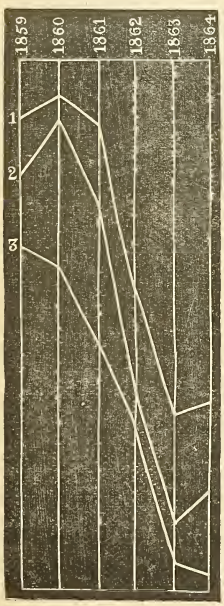


other, and that the annual values of the solar radiation might bear a constant ratio to the corresponding values of the oscillations of mean daily temperature. The annual sums and mean daily values of the oscillations of mean daily temperature at Oxford for the six years, and the corresponding ratios, were therefore calculated, and found to be as follows :—

	Annual Sums.	Daily Means.	Ratios.
	°	°	
1859	1182·3	3·24	3·96
1860	953·0	2·60	4·14
1861	1054·9	2·89	3·88
1862	1043·4	2·85	3·28
1863	1198·4	3·28	2·98
1864	1158·0	3·17	3·09

The numbers in the last column showed that the ratio was not even approximately constant, but that, on the contrary, it was subject to considerable change. I therefore concluded that the calorific intensity of the sun's light was also subject to variation; and a glance at the course of the numbers at once suggested that this variation would be found to follow that of solar spot frequency. Referring to Schwabe's observations of the solar spots, we have the following numbers of groups observed by him :—

B



1859	205	1862	160
1860	211	1863	124
1861	204	1864	130

A projection of these numbers, and of the corresponding ratios of solar radiation to the oscillation of mean daily temperature is shown in diagram B; No. 1 is the curve of sun spot frequency, and No. 2 that of the ratios; and it will be seen that the similarity

in form of the two curves is remarkably striking, and apparently conclusive, as to the connection between the two classes of phenomena.

Assuming that changes in the heating power of the sun's rays follow the course of the changes in solar spot frequency, it seemed probable that the ratio of the difference between the maximum temperature in the shade and in the sun, to the difference between the mean daily temperature,—or better, perhaps, the temperature of evaporation,—and the maximum temperature in the shade, would also exhibit corresponding changes. It will be seen that the following results strongly support this view :—

	Max. Temp. in Sun, less Max. Temp. in Shade. °	Max Temp. in Shade, less Mean Temp. of Evaporation. °	Ratio.
1859	12·85	10·06	1·27
1860	10·78	8·63	1.25
1861	11·24	9·52	1·18
1862	9·37	9·28	1·09
1863	9·78	10·38	0·94
1864	9·81	10·57	0·93

The line No. 3 in diagram B is a projection of these ratios. It would probably be better to employ the maximum instead of the mean temperature of evaporation, but this is not given in the printed observations.

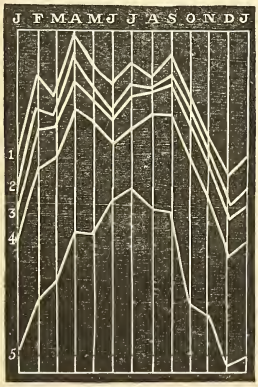
The mean values of solar radiation given above in the first table, are deduced from observations made on every day in the year, and therefore in every possible state of the atmosphere—clear, cloudy, rainy, foggy, calm, stormy, &c.—but it was evidently desirable to determine the calorific intensity of the sun's rays on days when the sky was cloudless at the time of maximum temperature. The printed observations, however, do not always show when this was the case, and it became necessary to adopt some arbitrary principle of selection. As the one which appeared to me to be least

open to objection, I selected, in the first instance, the highest value in each month of the six years, and taking the means obtained the following numbers, a projection of which is shown in curve No. 1 of diagram C.

	°		°
January	16·4	July	21·1
February	20·5	August	20·3
March	19·3	September	21·8
April	22·6	October	19·6
May	20·9	November	17·2
June	20·1	December	15·3

This curve exhibits two principal maxima in April and September, and a low minimum in December.

C



Taking next the means derived from the three highest values in each month, and then those of the five highest, we have curves No. 2 and No. 3. The slight irregularities in the first curve have now disappeared, and curve 3 has two well defined maxima in April and September, a principal minimum in December, and secondary minimum in June.

Proceeding now a step farther and taking the ten highest values in each month, we have curve No. 4. The first maximum still occurs in April, but a slight change has taken place in the time of the second, which now occurs in the latter part of August instead of the middle of September; but no change has taken place in the times of the two minima.

Finally, taking the ten lowest values in each month we have curve No. 5. Here the first maximum has almost disappeared, though still occurring in April, and the second

maximum has advanced into July. The principal minimum is still in December, but the secondary minimum occurs in May instead of June, though, like the first maximum, it has almost disappeared.

It seems therefore, from a comparison of these five curves, that the general curve in diagram A, which is laid down from the monthly means of all the observations, may be regarded as compounded of two primary curves,—one having two well marked maxima in April and September, and two minima in December and June, and the other having only one maximum in July and one minimum in December. From curves 3 and 4 it appears that the influence of the second primary curve upon the general features of the first, does not become apparent while the number of selected days is limited to five per month. The first primary curve therefore represents the monthly changes in the calorific intensity of the sun's direct rays on cloudless or nearly cloudless days, and it leads us to this remarkable conclusion that the heating power of direct sun-light on clear days in the latitude of the British Islands, is greater in the months of April and September than in the month of June, when the sun attains his greatest meridian altitude.

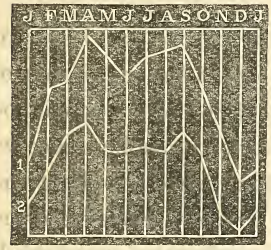
The second primary curve represents the intensities when the solar rays are more or less intercepted and dispersed by clouds and haze, and it approaches in form the annual curve of temperature; but the first curve, of which curve 3, diagram C, may be taken to be a fair representation, is unlike that of any other thermometric element. It has, however, a remarkably close resemblance to the curve representing the monthly changes of one of the magnetic elements, namely, that of the monthly means of the diurnal ranges of the magnetic needle. In the volume of the Greenwich Observations for 1859, the Astronomer Royal has given a table showing the monthly means of the diurnal ranges of the magnetometer at Greenwich from ten years'

observations, 1848–1857. The numbers in this table are as follow:—

January	9·5	July	12·7
February	11·3	August	12·6
March	13·1	September	13·5
April	14·0	October	12·2
May	12·7	November	9·3
June	12·6	December	8·2

The line No. 2, in diagram D, is a projection of these numbers, and the line No. 1 is a repetition of curve 3, diagram C. It will be seen that the maxima and minima of the one correspond exactly with those of the other, and we are therefore entitled to conclude that the two phenomena are intimately connected, and that the causes which produce variations in the intensity of solar radiation also affect the diurnal oscillations of the magnetic needle.

D

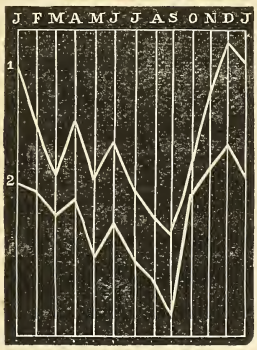


It was supposed by Sir William Herschel that the emission of heat from the sun varied according to the greater or less frequency of solar spots, and he attempted to support this view by a comparison of the prices of grain in years when the solar spots were numerous, with those in years when few or no spots were seen; but this kind of evidence was generally regarded as unsatisfactory, and it is, I believe, now generally admitted by meteorologists that the many valuable series of thermometrical observations which have since been made in various localities, have hitherto failed to afford any decided indications of a periodical change in the element of mean annual temperature. The question therefore naturally arises, if the intensity of solar radiation varies in a period corresponding with the period of solar spot frequency, why does not the element of mean annual tempera-

ture exhibit a similar periodical change? For the present, I will merely suggest that a clue to the correct explanation of this apparent anomaly may perhaps be found in the conclusion at which Professor Forbes arrived from a discussion of his observations made in Switzerland, in conjunction with Professor Kaemtz, on the intensity of solar radiation at different elevations in the atmosphere, namely, that the heating rays of the sun consist of two kinds,—one kind, of high intensity, which suffers little or no loss in passing through the atmosphere; and the other, of much lower intensity, and therefore much more absorbable. Now, if we suppose that the relative quantities of the two kinds of rays undergo periodical changes, such that the maximum of the one corresponds to the minimum of the other, it will be evident that when the lower strata of the atmosphere receive less heat by absorption, owing to a diminished supply of the rays of low intensity, the ground will receive more from the simultaneous increase in the quantity of the rays of high intensity, and this being communicated to the lower atmosphere by conduction and convection, as well as by radiation upwards, will restore the equilibrium, and tend to produce uniformity in the mean annual temperatures.

On a former occasion I urged the desirability of giving more attention than has hitherto been done to the oscillations of mean daily temperature, and have shown, in the present discussion, their importance in connection with the subject of solar radiation. I may now add that they appear to have an intimate connection with magnetic phenomena. Among the tables given by the Astronomer Royal in the volume of Greenwich observations for 1859, is one showing the monthly mean horizontal magnetic force at Greenwich for the nine years 1849–1857, corrected for secular variation. The line No. 2, in diagram E, is a projection of the numbers in this table, and the line No. 1, in the same diagram, shows the monthly mean daily

oscillations of temperature. It will be seen that every rise or fall in one curve has a corresponding rise or fall in the other. It is, in fact, rare to meet with so close an agreement between curves representing phenomena so widely different as the earth's horizontal magnetic intensity and the mean daily temperature of the atmosphere.



The conclusions arrived at from this discussion may be briefly recapitulated as follows :—

1st.—That the calorific intensity of the sun's light is subject to periodical changes, the maxima and minima of which correspond respectively with those of solar spot frequency.

2nd.—That the intensity of a ray of direct sunlight on its arrival at the earth's surface, in the latitude of Oxford, is greater in April and September than in June, when the sun's meridian altitude is greatest.

3rd.—That the curve representing the mean monthly values of solar radiation on cloudless days has its times of maxima and minima corresponding with those of the curve representing the mean monthly diurnal ranges of the magnetometer.

4th.—It seems probable that the heating rays of the sun consist of two kinds differing considerably in intensity, and being subject to periodical changes, the times of maximum of one kind, and those of minimum of the other, corresponding respectively to the times of maximum frequency of solar spots.

5th.—That the oscillations of mean daily temperature are intimately connected with the changes which take place in the earth's horizontal magnetic intensity.

I have said that the results derived from the Greenwich

observations of solar radiation were anomalous and unsatisfactory. It may, therefore, be necessary to state that these observations appear to have been made under circumstances not favourable to the accurate determination of the intensity of solar radiation at all seasons of the year. The values derived from them appear to be too high in summer, and too low in winter. Take, for instance, any winter month, say December, 1857. The mean difference for the month between the maximum in sun and the maximum in shade was only 1·7° : at Oxford, it was 6·1°. The highest value during the month was 4·5° at Greenwich : at Oxford, it was 19·0°. At Greenwich there were only seven days on which the difference exceeded 3·0° : at Oxford there were seventeen. And yet at Greenwich the month was unusually fine and dry, only 0·36 of an inch of rain fell, and several days appear to have been nearly, if not quite, cloudless. Under these circumstances, it is difficult to understand why the black-bulb thermometer, if properly exposed, did not register much greater differences. In 1859 there was a remarkable and unaccountable falling off in the summer values, and the mean for the year was decidedly lower than the mean of any of the three preceding years; but at Oxford it was higher. I can only account for this remarkable difference by supposing that some change was made in the position of the solar radiation thermometer at Greenwich in the early part of the year.

From the beginning of 1860 to the end of 1864 the observations at Greenwich were made with a black bulb thermometer in vacuo, and it is satisfactory to find that the course of the annual means is in tolerably fair agreement with that of the results obtained at Oxford with the ordinary black bulb thermometer.

"Solar Radiation Observations, made at Old Trafford, Manchester," by G. V. Vernon, F.R.A.S., F.M.S.

Mr. Joseph Baxendell, F.R.A.S., having at a recent meet-

ing of the Physical Section, read a paper on this subject showing some peculiarities of the distribution of Solar Radiation throughout the year, I have at his suggestion reduced and tabulated my observations bearing upon this point down to the end of 1866, making eleven years in all. Unfortunately the month of August is deficient in six years out of the eleven. The observations were made with Negretti and Zambra's patent maximum thermometers, neither thermometer having any index error, but reading correctly with the standard throughout the scale.

Taking the differences between the mean maximum black bulb reading in the sun, and the mean maximum reading in the shade for each month, we find a minimum in December and a maximum in July. The maximum occurs in the warmest month, but the minimum appears to occur some time before the coldest month arrives, viz.: January. If we take the differences between one month and the succeeding one we find the following values:

	°
January to February	+ 2·02
February to March	+ 4·64
March to April	+ 4·06
April to May	+ 1·67
May to June	— 0·09
June to July	+ 0·56
July to August	— 2·39
August to September	— 2·07
September to October	— 3·46
October to November	— 3·21
November to December	— 1·32
December to January	+ 0·59

From my observations the greatest increase in the amount of solar radiation appears to take place in March, and nearly as much in April, afterwards, up to July, the warmest month, the increase is remarkably small, and in June shows a slight amount of decrease. These figures do not quite

agree with those deduced from the Oxford observations by Mr. Baxendell, but they very clearly confirm the fact that a large amount of solar heat is dispersed in some way, and without a corresponding effect upon the thermometer, or the difference between the maximum in the sun and the maximum in the shade would show a large increase in the months of maximum temperature.

The greatest effect of solar radiation, therefore, appears to occur in the spring, and this is in accordance with the very rapid growth of vegetation we often see suddenly take place in early spring.

OLD TRAFFORD, MANCHESTER.—SOLAR RADIATION.

Year.	January.			February.			March.		
	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.
	°	°	°	°	°	°	°	°	°
1856	45·4	43·9	1·5	49·1	46·9	2·2	54·6	48·2	6·4
1857	43·1	41·7	1·4	49·9	46·4	3·5	54·4	47·7	6·7
1858	49·8	44·8	5·0	47·9	44·1	3·8	56·3	50·7	5·6
1859	51·1	46·0	5·1	53·6	47·4	6·2	57·6	50·8	6·8
1860	46·4	42·9	3·5	47·1	41·9	5·2	54·3	46·4	7·9
1861	41·3	40·4	0·9	52·2	46·8	5·4	60·4	49·6	10·8
1862	44·6	43·2	1·4	49·7	46·3	3·4	56·8	47·9	8·9
1863	...	...	...	55·2	49·6	5·6	62·8	51·6	11·2
1864	43·4	41·7	1·7	45·5	41·6	3·9	59·8	47·9	11·9
1865	43·6	41·3	2·3	46·8	42·3	4·5	57·0	44·1	12·9
1866	49·0	47·3	1·7	52·9	46·4	6·5	58·7	47·6	11·1
Means	45·77	43·32	2·45	49·90	45·43	4·47	57·52	48·41	9·11
	April.			May.			June.		
1856	66·3	55·9	10·4	70·6	59·7	10·9	76·4	64·3	10·1
1857	65·6	53·8	11·8	75·2	62·2	13·0	82·6	73·6	8·0
1858	66·5	57·9	8·6	72·6	59·5	13·1	82·7	73·8	8·9
1859	61·9	53·9	8·0	79·4	65·7	13·7	83·5	68·8	14·7
1860	63·0	54·4	8·6	81·7	65·4	16·3	81·3	63·9	17·4
1861	72·6	55·5	17·1	73·2	59·4	13·8	86·2	68·6	17·6
1862	70·8	56·0	14·8	81·2	62·7	18·5	...	...	...
1863	69·9	56·2	13·7	74·8	59·5	15·3	81·4	65·0	16·4
1864	76·0	59·0	17·0	81·4	66·5	14·9	81·2	66·2	15·0
1865	81·5	63·5	18·0	79·3	65·0	14·3	93·0	72·8	20·2
1866	74·1	57·3	16·8	82·1	62·2	19·9	88·0	71·8	16·2
Means	69·84	56·67	13·17	77·37	62·53	14·84	83·63	68·88	14·75

SOLAR RADIATION—(Continued).

	July.			August.			September.		
	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.
	°	°	°	°	°	°	°	°	°
1856	80·7	68·0	12·7	...	...	...	74·3	63·9	10·4
1857	83·6	69·6	14·0	82·5	72·0	10·5	75·8	67·4	8·4
1858	79·6	66·8	12·8	...	...	...	79·7	66·3	13·4
1859	86·7	74·2	12·5	...	...	...	76·2	63·7	12·5
1860	83·8	66·8	17·0	...	...	...	71·8	59·5	12·3
1861	85·8	67·1	18·7	...	...	...	78·5	63·9	14·6
1862	80·3	64·8	15·5	...	...	...	74·2	62·6	11·6
1863	87·3	68·9	18·4	83·4	66·0	17·4	70·1	58·5	11·6
1864	86·0	70·9	15·1	80·9	68·0	12·9	73·3	65·2	8·1
1865	94·4	76·6	17·8	81·7	68·7	13·0	82·3	73·4	8·9
1866	84·2	70·3	13·9	77·8	67·0	10·8	70·0	62·4	7·6
Means	84·76	69·45	15·31	81·26	68·34	12·92	75·11	64·26	10·85

	October.			November.			December.		
1856	66·3	59·4	6·9	47·6	45·7	1·9	45·4	44·8	0·6
1857	66·9	59·8	7·1	56·5	50·1	6·4	57·5	50·8	6·7
1858	63·8	55·0	8·8	51·8	46·6	5·2	50·2	45·3	4·9
1859	66·1	56·5	9·6	51·5	46·9	4·6	42·6	39·8	2·8
1860	64·1	55·4	8·7	49·0	45·3	3·7	40·8	39·5	1·3
1861	64·8	60·6	4·2	47·2	45·9	1·3	44·0	44·8	–0·8
1862	61·9	56·5	5·4	44·9	43·7	1·2	48·2	48·0	0·2
1863	62·0	55·9	6·1	52·3	50·7	1·6	48·6	48·3	0·3
1864	61·7	57·0	4·7	49·1	48·2	0·9	41·9	43·2	1·3
1865	62·4	58·6	3·8	51·7	50·5	1·2	47·8	47·4	0·4
1866	62·5	58·1	4·4	56·4	49·5	6·9	52·8	47·5	5·3
Means	63·86	57·47	6·39	50·73	47·55	3·18	47·26	45·40	1·86

MONTHLY VALUES.

	Max. in Sun.	Max. in Shade.	Diff.
	°	°	°
January	45·77	43·32	2·45
February	49·90	45·43	4·47
March	57·52	48·41	9·11
April	69·84	56·67	13·17
May	77·37	62·53	14·84
June	83·63	68·88	14·75
July	84·76	69·45	15·31
August	81·26	68·34	12·92
September	75·11	64·26	10·85
October	63·86	57·47	6·39
November	50·73	47·55	3·18
December	47·26	45·40	1·86

"A Comparison of Solar Radiation on the Grass, and at Six Feet from the Ground," by THOMAS MACKERETH, F.R.A.S., F.M.S.

As the Corporation of the Borough of Salford has recently erected a structure in front of the Town-Hall for meteorological purposes, and as no better position could be found for the solar radiation thermometer than on the apex of the roof of the shade-stand, which is six feet from the ground, I fixed a similar thermometer in a similar position at Eccles in order that I might institute a comparison of solar radiation between Eccles and the Borough. Though I anticipated very different results from a thermometer thus placed, than I had found from a similar thermometer placed upon the grass, I was not prepared to find them so widely different as I shall here present them. The instruments used are the ordinary exposed black-bulb self-registering thermometers, duly compared. I have also added a column of comparison from results obtained by a self-registering black-bulb thermometer placed *in vacuo* on the grass. The observations are for the month of October only, the only complete month in which all the instruments have been used.

DATE.	Exposed Black Bulb Therm. on Grass.	Exposed Black Bulb Therm. 6 feet from ground.	Difference.	Black Bulb Therm. *in vacuo* on grass.	Difference from Therm. 6 feet from ground.
1867	°	°	°	°	°
Oct. 1	69·2	73·0	+ 3·8	83·2	+10·2
„ 2	57·5	57·6	+ 0·1	62·1	+ 4·5
„ 3	61·4	62·7	+ 1·3	71·0	+ 8·3
„ 4	52·5	57·0	+ 4·5	60·0	+ 3·0
„ 5	66·5	68·2	+ 1·7	72·2	+ 4·0
„ 6	64·0	68·0	+ 4·0	67·7	— 0·3
„ 7	60·7	67·2	+ 6·5	77·4	+10·2
„ 8	54·9	60·1	+ 5·2	68·0	+ 7·9
„ 9	46·7	45·8	— 0·9	48·9	+ 3·1
„ 10	62·0	67·6	+ 5·6	75·8	+ 8·2
„ 11	51·3	52·5	+ 1·2	54·0	+ 1·5
„ 12	56·2	56·5	+ 0·3	56·5	0·0
„ 13	53·4	52·7	— 0·7	57·0	+ 4·3
„ 14	60·0	61·5	+ 1·5	61·2	— 0·3
„ 15	61·0	63·6	+ 2·6	65·0	+ 1·4
„ 16	64·8	74·3	+ 9·5	71·0	— 3·3
„ 17	63·0	74·8	+11·8	71·1	— 3·7
„ 18	62·5	70·7	+ 8·2	67·5	— 3·2
„ 19	57·3	68·2	+10·9	62·0	— 6·2
„ 20	60·5	68·9	+ 8·4	64·4	— 4·5
„ 21	60·0	63·8	+ 3·8	63·1	— 0·7
„ 22	72.8	82·1	+ 9·3	79·8	— 2·3
„ 23	64·0	64·8	+ 0·8	65·6	+ 0·8
„ 24	54·2	55·4	+ 1·2	56·0	+ 0·6
„ 25	51·8	52·5	+ 0·7	53·0	+ 0·5
„ 26	62·0	69·3	+ 7·3	65·3	— 4·0
„ 27	50·3	51·4	+ 1·1	53·7	+ 2·3
„ 28	52·6	61·9	+ 9·3	58·7	— 3·2
„ 29	59·4	65·4	+ 6·0	62·0	— 3·4
„ 30	61·0	68·8	+ 7·8	67·0	— 1·8
„ 31	58·3	64·8	+ 6·5	64·2	— 0·6
Mean...	59·0	62·9	+ 3·9	64·6	+ 1·7

It appears therefore that, for the month, at a position six feet above the ground, there is a mean difference of solar influence of nearly 4 degrees above that upon the grass; and in some extreme instances the difference has been from 10 to nearly 12 degrees. A thermometer placed *in vacuo* on the grass has only exceeded this difference by 1·7 degree. These results show how important it is that some definite principle should be adopted in the placing of solar thermometers, as certainly no comparison can be made between the amount of solar radiation at any two or more places, unless some common plan of placing the instruments be adopted.

PHYSICAL AND MATHEMATICAL SECTION.

October 10th, 1867.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"On Solar Radiation," by J. BAXENDELL, F.R.A.S.

[This paper was afterwards read at the Ordinary Meeting of the Society, held November 26th, 1867, see page 36.]

November 7th, 1867.

ROBERT WORTHINGTON, F.R.A.S., President of the Section, in the Chair.

"A Comparison of Solar Radiation on the Grass, and at Six feet from the Ground," by THOMAS MACKERETH, F.R.A.S.

[This paper was afterwards read at the Ordinary Meeting of the Society, held November 26th, 1867, see page 50.]

"Note on the Colour of the Moon during Eclipses," by A. BROTHERS, F.R.A.S.

On the night of October 4th, 1865, there was a partial eclipse of the moon, when about one-third of the disc was obscured. My time and attention on this occasion were chiefly directed to some Photographic experiments, and it will be remembered I then obtained pictures of the eclipse at short intervals, from the commencement to the end. At about the greatest phase I looked at the moon through the telescope with an eye piece of low power, for the purpose of noticing whether the eclipsed portion showed colour, and at once saw that the part of the moon most deeply within the shadow was of a decided copper colour, such as I had seen some years previously during a total eclipse of the moon.

The eclipse which occurred on the night of the 13th September last, was rather more favorable for detecting the presence of colour, as seven-tenths of the moon were covered by the earth's shadow; the weather being clear many persons have recorded their observations, and as the

colour of the moon is one of the chief features of a lunar eclipse, this point attracted considerable attention.

Mr. Browning says, in a letter which appeared in the Astronomical Register—"I looked most carefully for colour both with the $10\frac{1}{2}$ silvered glass reflector furnished with an achromatic eyepiece of very low power, and also with a five-feet refractor; with neither could I detect a trace."

Mr. Slack who was also observing with a silvered glass reflector, and in the same locality as Mr. Browning says,—"After 12, the eclipsed limb grew noticeably redder, the red coppery tint chiefly affected the lower parts of the obscured limb, but was visible further in, gradually blending with the inky tints presented by the umbra at its advancing edge.—(Int. Obs. October.)

Mr. Weston, observing at Landsdown, near Bath, says,—"The prevailing colours were red-bluish and grey, *and grey*; the redness increased towards the darkened edge of the moon."—(Monthly Not. 9—xxvii.)

Many other observers speak of the presence of colour, but on the other hand a few say they did not notice any, the eclipsed portion of the moon merely having a darkened appearance.

On this occasion I did not make any Photographs, as I could not expect results materially differing from the last, and I gave my whole attention to observing the progress of the eclipse through the telescope, which is a refractor of 5 inches aperture. As to the question of the presence of colour, I can most distinctly say that *colour* gave the moon a very beautiful appearance, and it seemed to me the most interesting feature of the eclipse. The beauty of the moon's surface appeared to increase as the penumbral shadow stole over its surface, and until the shadow itself was considerably advanced, all the details of the lunar surface could be distinctly made out, and during the whole period of the eclipse some of the brighter points of light within the shadow continued visible, as did also the entire disc with

many of the details of light and shade. The colour of the eclipsed limb was of a coppery hue, much brighter towards the part most deeply within the shadow. The part of the moon not eclipsed was of a beautiful bluish-grey colour.

That the appearance of colour cannot be caused by the telescope or by peculiarities in the eyes of the observers is, I think, proved by the fact, that the same colours are seen whether refractors or reflectors, either of metal or silvered glass, be used; and, as the majority of observers see colour, the eyes of those who remark the absence of it are perhaps temporarily afflicted with colour blindness;—the bright light from the uneclipsed portion of the moon may be sufficient to produce this in some persons. If an observer, after looking at the moon through the telescope, attempts to look at objects while the other eye remains closed, it will be found that until the retina has recovered from the excess of light everything will appear misty—in fact, the eye is partially blinded, and it may be that some eyes are sufficiently sensitive to be affected by the diminished lustre of the moon, and may thus be prevented seeing colour, which there can be no doubt the lunar surface presents during an eclipse.

Some observers have remarked on the difficulty of detecting the first appearance of the shadow, and although the exact time is known, the real shadow is not seen until many seconds or perhaps minutes after the predicted time. In the present instance, I watched very carefully for the first appearance of the shadow, and, having previously ascertained the error of my watch, I took my position at the telescope, at the same time I requested a friend to take particular notice of the time at the moment I saw the shadow. The result thus obtained was within twenty seconds of the time given in the Nautical Almanac, clearly showing that under favourable conditions, and, if the attention be given carefully to the subject, the real shadow may be detected very near the predicted time.

Ordinary Meeting, December 10th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Mr. Samuel Broughton was elected an Ordinary Member of the Society.

"On the Age of the Hæmatite Iron Deposits of Furness," by E. W. BINNEY, F.R.S., F.G.S.

The great value of the vast deposits of hæmatite iron ore found in the Carboniferous Limestone of North Lancashire, and in a similar position near Whitehaven, in Cumberland, renders any information as to the date of their formation of considerable importance. There is no doubt that the Carboniferous Limestone was first formed in most instances. Hollows were then made by some agency, whether by aqueous or atmospheric, or both agencies, it is not necessary here to inquire; but most of the cavities bear evidence of some erosive action, their sides being more like old valleys or watercourses than simple fissures. The next question to be considered is when were these hollows filled with the iron ore? No one who has investigated the subject would estimate their age so late as the date of the Magnesian Limestone, but some eminent geologists have supposed them to have been formed during the Lower Permian period, whilst others have adduced reasons for believing that they had been formed during the Carboniferous epoch. There is very little doubt but that large deposits of iron have been met with in both these formations, some of Carboniferous and others of Permian age, but our present enquiry is the geological age of the hæmatite iron ores of Furness.

In two memoirs printed in the Transactions of this Society, namely, in Volumes VIII. and XII. (second series), he had described deposits of hæmatite iron ore, and given his reasons for believing that they had their origin during the Carboniferous epoch, and were deposited in erosions of the Mountain Limestone in some instances, or in others interstratified in regular beds in the Lower Coal Measures like seams of coal. Long after his papers had been printed, Sir R. I. Murchison, Bart., F.R.S., and Professor Harkness, F.R.S., in a Memoir on the Permian Rocks of the North West of England, printed in Volume XX. of the Quarterly Journal of the Geological Society, at page 152, say, "The mode in which that valuable ore of iron *hæmatite* is found deposited in pre-existing cavities of the carboniferous formation, and sealed up by Crab Rock (Permian breccia) is a matter of great geological interest. Joints, fissures, and caverns were doubtless formed in the older rocks antecedent to the deposition of the Permian strata, and in these the ores of iron, so widely diffused throughout the Permian rocks, have in this portion of the north-western region assumed the character of hæmatite. This circumstance justifies the inference that these hæmatite ores are the result of an agency which ushered in the Permian epoch. The earlier rocks of both England and Scotland are strongly impregnated with iron, their composition consisting principally of silica and an oxide of this metal. This latter substance originated from the same source which, during the commencement of the deposition of the Penrith sandstones, filled up the fissures in the Carboniferous Limestone. This conclusion is applicable not only to the Ulverstone district but also to that of Cleator, south-east of Whitehaven, where valuable deposits of hæmatite are also obtained from the cavities and fissures in the Carboniferous Limestone, which at one time was here covered over by an extension of those Permian breccias and sandstones, now forming an

escarpment a short distance west of Cleator Moor. This inference concerning the Permian age of the hæmatite had been arrived at by Professor Phillips, who, in a communication to the British Association, of which a short abstract was published (Report of Brit. Ass. 1858, Trans. Sects. p. 106), illustrated his opinions by maps and drawings."

The authors of the paper illustrated their views in a section from Park to Furness Abbey, shown in a woodcut exhibiting two vertical fissures in the limestone rock, filled with hæmatite and covered up with breccia or crab rock. Now this undoubtedly shows that the limestone rock was first formed, then that the fissures or erosions were made and subsequently filled with hæmatite. But at what time the latter process took place, whether during the Carboniferous or Permian epoch no evidence is adduced by the authors further than that the beds of hæmatite are there covered up by Permian breccia, and some of the latter has been introduced into the fissures. It has been generally supposed by geologists that at the close of the Carboniferous epoch great convulsions took place in the earth's crust, and large quantities of iron were ejected from its interior. Now, the three authors previously named are recognised authorities on the age of hæmatite iron ores, and their opinions deserve great attention. When his papers were written, conclusive evidence could only be given of the age of the hæmatite iron of Ipstones, in Staffordshire, which was clearly interstratified in the Lower Coal Measures between the Rough Rock or Upper Millstone of Professor Phillips and the Geological Survey and the Gannister coal. Some years since, Mr. Bolton, of Swarthmoor, near Ulverstone, showed him, amongst other fossils, a beautiful specimen of *Sigillaria vascularis*, exhibiting both its external characters and internal structure, quite as perfect in every respect as the specimens found in the Gannister or Hard coal at Halifax or the Bullion seam of Burnley, all converted into hæmatite

iron. At that time no doubt existed in his mind of its having come from one of the Furness iron mines, but Mr. Bolton could give him no proof of the exact locality where it was found. At a meeting of the Manchester Geological Society, held on the 30th May, 1866, he gave his opinion of the value of this fossil as indicating the carboniferous age of the hæmatite iron ores of Furness. These specimens have been kindly lent him by Mr. Swainson, of Newland, their present owner, and were exhibited on the table for examination. With them were some other interesting specimens from the cabinet of Miss E. Hodgson, of Ulverstone, a lady well known for her acquaintance with the geology of Furness, and who procured the specimens now exhibited from the Water Blain Mines, west of the Duddon. They do not exhibit their external characters so well as Mr. Swainson's specimens do; but one is a *Stigmaria*, the root of *Sigillaria*, and another a *Lepidodendron*, two common coal plants, which indicate the carboniferous age of the deposit in which they were found as clearly as any fossil organic remains can do. There is no doubt about the locality of Miss Hodgson's specimens. They came from the Water Blain Mines, and plenty more may be obtained from the same place. They are all converted into good hæmatite iron, that substance having metallized them in a similar way, as we find plants in the Coal Measures converted into the carbonate of the protoxide or the bisulphide of iron or carbonate of lime.

It was not his intention here to attempt to give any explanation of the process by which the original ligneous structure of the plant has been removed and replaced by per-oxide of iron, that is a subject better fitted for the chemist than the geologist.

Now the discovery of common coal plants not only embedded in but actually formed of hæmatite iron, surely indicates the carboniferous age of the deposits in which

they are now found as clearly as it well can be, for the plants must have been floated in with the water which brought the iron, or else they must have fallen into the cavities when such were open at the top, and the iron was in a soft state. Of course, the occurrence of such fossil plants as *Sigillaria, Lepidodendron* and *Calamitis* occurring as they do in beds, from the lowest to the highest carboniferous strata, would give little evidence of any particular part of that epoch, but the *Sigillaria vascularis* so far as yet known is confined to the lower coal-field, not far in geological position from the Ipstones hæmatite previously alluded to, and the valuable clay band ironstones now wrought at Hazlehead, west of Penistone.

The deposits of hæmatite in Furness and Cumberland, found in hollows of the Carboniferous Limestone, and covered up by Till, or "Pinel," as it is locally termed, or more rarely by Permian breccia, are so much alike in all their characters that if the origin and age of one of them are clearly proved those of the rest must follow almost as a necessary consequence.

W. Brockbank, F.G.S., agreed with Mr. Binney, that the fossils now exhibited were valuable illustrations of the age of the deposits of hæmatite ore in the Furness district; but they did not afford any evidence of their origin, and were merely brought there with the other alluvium, and deposited with the iron ore, in the cavities of the limestone. The Cumberland district afforded better opportunities for studying the origin of these ores. True veins of hæmatite occur in the older slates, porphyries and syenites of the Lake district. Red Pike derives its name from the presence of veins of this ore, which gives the mountain its ruddy aspect; and iron appears to have been smelted near it, in the time of the Romans. On Black Comb, Dent Fell, and in the valleys above Ravenglass, veins of hæmatite occur in the older rocks, and are worked

to a small extent, and the ore carted down to the railway. The richest deposits however occur in those places where clay slate is overlaid with other rocks, and principally in the carboniferous limestone.

In the Cleator district a narrow band of limestone, resting upon the clay slate, curves around the flank of Dent Fell, and in this narrow zone most of the iron ore mines of the Whitehaven district occur. The ore is found generally in large masses or pockets, in the body of the limestone, sometimes in contact with the clay slate, and at others filling cavities and clefts to the surface, and covered by alluvium. The limestone is at places overlaid by whirlstone or millstone grit, and the ore occurs both above and below this rock. In one remarkable mine the ore fills a large cavity in the limestone, and is covered by whirlstone, which forms a perfectly even roof over a large area. Where the hæmatite occurs near the clay slate, it is generally hard and crystalline, and it is much softer and less siliceous where it appears to have been deposited in the limestone, or by alluvium, causing a classification in this district into hard and soft ores, whereas in the Furness district they are generally of the soft kind.

In considering the origin of these ores, we must go back to the time when the limestone and associated rocks of the Carboniferous period had been formed at the bottom of the sea, and were raised therefrom by some vast force of upheaval which would naturally cause great denudation and break up the rock into clefts and fissures. This would appear to have been a time of great volcanic or at any rate of metalliferous activity, and the two mountains of Dent Fell and Black Comb bear evidences of having been very active centres of this action, and their upheaval may very possibly have been connected with the eruption of the hæmatite ores. It is therefore probable that during the early part of the Carboniferous era, the hæmatite ores were

formed by these agencies, and by the denudations, which were vast and probably frequent, were partly removed and deposited in clefts and cavities wherever they were carried. The fossils produced by Mr. Binney point also to this period in the Furness district, as that in which the ores were carried into the cavities they now fill. The two mountains of Dent Fell and Black Comb appear to stand as a line of hæmatite ore mines, the country both between them and on their line north and southwards being dotted over with mines, and being most productive in their immediate vicinities. At Hodbarrow, immediately at the foot of Black Comb, occurs one of the richest mines yet discovered. It lies in a cleft of limestone, and appears to be more like a lode or vein than a deposit, and is immediately upon the shore of the Duddon estuary. This vein has been found on the south shore of the estuary just opposite, and it is fully believed that it is continuous through the sea. The richest mines of the Furness district are also immediately to the south of this point. The principal mines of Furness are deposits, as shewn by Mr. Binney, and the evidence of the Cleator district would point to Black Comb as a very possible source for their origin.

In the Cleator district the Carboniferous strata are at places overlaid unconformably by the Permian of St. Bees Head, and in the lower red sandstone, underlying the magnesian limestone and conglomerate, are found many fossil plants, which are generally coated with red ore instead of carbonaceous matter. The conglomerates of St. Bees and Rougham Point also contain pebbles of hæmatite; both facts pointing again to denudation of hæmatite ore during the Permian era.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 2nd, 1867.

A. G. LATHAM, ESQ., in the Chair.

Mr. DANCER sent for exhibition a Revolving Platform for the Microscope, similar to one shown by Mr. HEPWORTH some years ago at a meeting of the Section; the one now exhibited has been patented in London. He also showed a Paper Slide Cabinet, invented by Mr. PIPER, of London.

"On Plants appearing in successive years on land prepared for Plantations, in Cheshire," by H. A. HURST, Esq., and GEORGE CARTER, Esq,

The struggle for existence between man and his fellow-man hourly going on in all populous countries has long been the theme of the historian, and forms the subject matter of the science of political economy. It is, however, but recently that Mr. Darwin, in his great work on the origin of species, has shown that this same struggle is proceeding with perhaps greater intensity in the animal and vegetable kingdom. With greater intensity because moral force, which mitigates the severity of the conflict among the human race, is, as far as we know, wanting in purely animal and vegetable organisms. Man would recoil from destroying an aged pauper merely because he was useless; but a tree must necessarily destroy most plants growing under its shade. It has no power to do otherwise. Still, the truth of this theory is only partially admitted and its full effect hardly ever realised even by scientific men.

We attribute this, in a great measure, to the recent date of its promulgation and the comparatively few facts bearing on the subject which have been hitherto accumulated. Like Newton's great ideas, it will be left to the future patient student of the facts of natural history to discover the many

proofs existing of its accuracy. As the discovery of Neptune by Adams and Le Verrier demonstrated the theory of gravitation, so probably at some future day will the truth of Darwin's assertion of the antagonism, as opposed to the so-called peace, existing in nature be proved by the prediction of some fact in botany of similar importance. In the meantime, it is the duty of all lovers of natural history to go on quietly accumulating facts bearing on the subject; and we therefore submit to your consideration the following observations, not as being in themselves of any value, but as affording an instance of this great struggle for existence going on in the vegetable world observed under unusually favourable conditions.

In Tatton Park, Knutsford, it has been the practice for the last few years to dig up, enclose, and plant with young trees each December, about an acre of land.

The size of the trees planted is so small that the effect on the vegetation around may, at least for some years, we think, be neglected without any error of magnitude being introduced into our results. Hence we have plots of land placed before us at intervals of one, two, three years in a state of nature—the disturbance of deer, cattle, &c., and especially man, carefully guarded against. These appear to be favourable circumstances for the discovery, if not of the indigenous plants, at all events of those which would eventually obtain possession of the land of the district, were it left in a state of nature.

We have not thought it necessary to go farther back than four years, because at that period the question appears to be decided, and the trees artificially planted had then acquired a height which would affect the conditions of the problem.

The plots were examined at two periods—January and the end of July, 1867.

The site is on what is called in the Ordnance Geological Survey, the Upper (Keuper) Red Marl of the New Red

Sandstone, and has an easterly aspect. The ground is dug two spades deep, and is a light sandy soil.

The young trees planted in each plot comprise Larch, Spruce, Scotch Fir, Oak and a few Sycamore.

No. 1. *Cleared January*, 1867.

19. Ranunculus acris, } few plants.
20. „ repens, } few plants.
21. „ bulbosus, } few plants.
63. Capsella Bursa Pastoris.
136. Viola arvensis.
218. Hypericum humifusum.
267. Trifolium repens.
332. Potentilla Tormentilla.
588. Taraxacum officinale.
643. Senecio vulgaris.
671. Achillea Millefolium.
800. Anagallis vulgaris, red flower, in great abundance.
914. Chenopodium album, one plant.
934. Polygonum lapathifolium.
939. „ aviculare.
941. „ convolvulus.
952. Rumex Acetosella.
1162. Juncus bufonius.
1186. Scirpus setaceus, one plant.
1313. Holcus lanatus, } tufts here and there.
1314. „ mollis, } tufts here and there.

21 species. The ground not one eighth covered.

No. 2. *Cleared January*, 1866.

88. Arabis Thaliana.
136. Viola arvensis.
168. Sagina procumbens.
185. Stellaria media.
189. „ uliginosa.
193. Cerastium triviale.
218. Hypericum humifusum.
267. Trifolium repens, more numerous than in No. 1.
282. Trifolium minus.
283. Lotus corniculatus.
291. Ornithopus perpusillus.
297. Vicia Cracca.
332. Tormentilla potentilla.
517. Galium saxatile.
588. Taraxacum officinale.
671. Achillea Millefolium.
675. Campanula rotundifolia.
756. Veronica officinalis.
778. Digitalis purpurea.
844. Prunella vulgaris.
952. Rumex Acetosella, in large quantity, disputing the ground with the Holci.
1251. Carex prœcox, single plant
1271. Anthoxanthum odoratum, scarce.
1291. Agrostis vulgaris.
1313. Holcus lanatus, } occupying equal ground with Rumex acetosella.
1314. „ mollis, } occupying equal ground with Rumex acetosella.
1528. Poa annua.
1362. Lolium perenne, scarce.

28 species. Holcus lanatus, mollis and Rumex acetosella, the predominant plants, struggling together for supremacy.

No. 3. *Cleared January*, 1865

In 1866 Galium saxatile was a very prominent plant.

21. Ranunculus bulbosus.
283. Lotus corniculatus.
332. Potentilla Tormentilla.
340. Rubus fruticosus.
517. Galium saxatile.
671. Achillea Miliefolium.
750. Veronica serpyllifolia.
756. Veronica officinalis.
952. Rumex acetosella, struggling for existence against the Holci.
1151. Juncus effusus.
1313. Holcus lanatus.
1314. ,, mollis.

12 species. Here the Holci are acquiring possession of the ground to the exclusion of other species. R. acetosella alone making a feeble struggle against them.

No. 4. *Cleared January*, 1864.

21. Ranunculus bulbosus.
283. Lotus corniculatus.
332. Potentilla Tormentilla.
339. Rubus Idœus.
340. ,, fruticosus.
588. Taraxacum officinale.
778. Digitalis purpurea.
944. Rumex crispus.
952. ,, Acetosella.
1151. Juncus effusus.
1291. Agrostis vulgaris.
1313. Holcus lanatus.
1314. ,, mollis.
1339. Dactylis glomerata.

14 species. Here Holcus lanatus and mollis cover more than seven-eighths of the ground, the next plant in number being Rumex Acetosella, the specimens of which are poor and stunted, the species evidently unequal to struggle with the Holci.

The total absence of Umbelliferæ in all these plots is curious, especially as the biennial and perennial plants, Rubus fruticosus and Digitalis purpurea appear in the last plot. Perhaps the Umbelliferæ require the presence of shade.

From the above lists of plants, it follows that the first occupier of the ground in mass is Rumex Acetosella, and that it continues the struggle for two years with Holcus mollis and lanatus, which two species or varieties finally usurp the ground, destroying all competitors with the exception of a few single representatives chiefly of biennial plants.

The absence of any species of Fumaria and Papaver so common, nay almost universal, in similar situations, appar-

ently points to the agency of man in distributing these plants, as it is the only condition wanting.

The nomenclature here adopted is that of the London Catalogue of British Plants of 1867, printed by Robert Hardwicke.

In our remarks we have considered the whole four plots as if they were one observed at different periods; but it is obvious that this is not absolutely correct, but that subsequent investigations will be required to confirm our conclusions.

Mr. J. SIDEBOTHAM wrote, stating that Polygonum convolvulus was the only plant which grew from soil obtained from under the foundations of a wall built some thirty years ago at Bowdon.

Ordinary Meeting, December 24th, 1867.

E. W. Binney, F.R.S., F.G.S., Vice-President, in the Chair.

"Memorandum on the Evaluation of Integrals," by the Hon. Chief Justice Cockle, F.R.S., &c., of Queensland. Communicated by the Rev. Professor Harley, F.R.S.

The processes indicated in my paper "On the Conversion of Integrals"* admit of direct application to certain results which Boole attained by a contemplation of the researches of Mr. Harley† and of Mr. Cayley. The object of this memorandum is to give a more general form and perhaps a more convenient expression to a portion of the processes of that paper. Thus we may generalise and restate (7) by saying that if

$$\mathrm{A}v\frac{d^3\phi}{dvdx^2}+\alpha\frac{d^2\phi}{dx^2}+\beta v\frac{d^2\phi}{dvdx}+\gamma\frac{d\phi}{dx}+\epsilon v\frac{d\phi}{dx}+\zeta\phi$$

wherein A, α, β, . . ζ are functions of x only and free from v, be, when v is treated as the only independent variable, a perfect differential coefficient with respect to v of a function f_2, then $\int\phi dv$, which for the sake of brevity we may replace by y, satisfies the linear differential equation of the second order

$$(\alpha-\mathrm{A})\frac{d^2y}{dx}+(\gamma-\beta)\frac{dy}{dx}+(\zeta-\epsilon)y+\mathrm{A}v\frac{d^2\phi}{dx^2}+\beta v\frac{d\phi}{dx}+\epsilon v\phi=f_2+\mathrm{X}\ldots(1)$$

where f_2 is obtained, where obtainable, by properly assigning

* *Phil. Mag.* Suppl., July, 1867, p. 537. Art. 9 may be expunged as involving an error of computation, and Art. 10 is susceptible of a perhaps more general expression.

† Mr. Harley and I have since been conducted independently to a generalization of one of Boole's results, which consists in applying Boole's process to forms of trinomial equations other than that discussed by him. I think it right to say that Mr. Harley's communication to me on the subject included his demonstration of his generalization, mine to him a verification (or perhaps two) only.

the five ratios of the six indeterminates A, α, β, .. ζ and wherein X is a function of x only which vanishes when the integration with respect to v is definite. If we put $A = \alpha$ then (1) becomes a linear differential equation of the first order and we have still four disposable ratios.

Oakwal, near Brisbane, Queensland,
Australia, October 17, 1867.

"On some Points in the Restoration of Euclid's Porisms," by T. T. WILKINSON, F.R.A.S., Corresponding Member of the Society.

M. Chasles, in his *Aperçu Historique*, p. 282, says that: "Diophante dans ses *Questiones Arithmetiques*, a plusieurs fois employè le mot Porisme,* pour désigner certain propositions concernant la théorie des nombres, sur lesquelles il appuie ses démonstrations, et qui formaient probablement un ouvrage qui ne nous est pas parvenu. Voir, par exemple, les Propositions 3, 5, et 19 du Livre V." This very valuable work was published at Brussels in 1837. The same Author in his restoration of *Les Trois Livres de Porismes D'Euclide*, published at Paris in 1860, gives an analysis of these Porisms in pp. 47—51, and adds to their

*"Le Porisme est une proposition dans laquelle on énonce une vérité, en affirmant qu'on peut toujours trouver certaines choses qui la complètent." —(Chasles, *Porismes* p. 54.)

This *new* definition regards Porisms as *incomplete Theorems*.—M. Breton (*Porismes*, 1866), regards them rat heras *incomplete Problems*.

"A Porism proposes to demonstrate that one or more things may be found, between which and innumerable other objects assumed after some given law a certain specified relation is to be shewn to exist."—(Leslie, *Geom. Anal.*)

"A Porism is a proposition affirming the possibility of finding such conditions as will render a certain problem indeterminate, or capable of innumerable solutions."—(Playfair, *Edin. Trans.*)

"Porisma est propositio in qua proponitur demonstrare rem aliquam, vel plures datas esse, cui, vel quibus, ut et cuilibet ex rebus innumeris, non quidem datis, sed quæ ad ea quæ data sunt eandem habent rationem, convenire ostendendum est affectionem quandum communem in propositione descriptam."—(Simson, *Opera.*)

"Porisma vero, quod proponitur in porismum, hoc est in inventionem et investigationem propositi."—(Commandine, *Pappus*, Vol. II, p. 160.)

number Prop. 17 and 20, Book III; Prop. 20 and 44, Book IV; and lastly Prop. 12, Book VI.

In this *résumè* of the "Porismes cites par Diophante" he again refers to the lost treatise from which Diophantus is supposed to have quoted; and he here claims to have been the *first* who had fully understood the nature of these properties of numbers.

He there says:—"Diophante ne parle pas expressément des Porismes, comme Proclus, et n'a pas à les définir. Mais il cite dan ses *Questions Arithmetiques* sous le titre de *Porismes*, des propositions extraites d'un ouvrage, apparemment d'un *Recueil de Porismes*, qui ne nous est pas parvenu. Ces propositions, auxquelles je crois que l'on n'avait jamais fait attention, du moins à titre de *Porismes* dans le sens d'Euclide, *avant que nous les eussions signalées* dans l'*Aperçu Historique*, se rapportent aux propriétés des nombres; et ce qui a de l'intérét ici, c'est que, sons le nom de *Porismes*, elles ont dans leurs énoncés la forme des *Données*, la même que nous attribuons aux *Porismes*."

This point is of some importance in the history of the restoration of the Porisms; and hence M. Chasles's claim to this discovery cannot be allowed. I possess a copy of a letter (the original is in the *Chetham Library*), from the Rev. Charles Wildbore, sometime editor of the *Gentleman's Mathematical Diary*, to the Rev. John Lawson, then Rector of Swanscombe, Kent, and brother of the Rev. Charles Lawson, Head Master of the Grammar School, Manchester, which announces the same discovery. The essential portions of this letter, and of the one which gave rise to it, will render this sufficiently clear. Mr. Wildbore was a geometer of a very high order,* and he had been engaged on Porisms before it became known that Dr. Simson had restored them. Mr. Lawson in a letter to him, dated "August 10th, 1775," says "I have very important

*See his investigation of "Lawson's Theorems," in the Manchester *Memoirs* Vol. II. N.S. pp. 414—452.

news to tell you, which I should much rejoice at, were it not for one consideration, which is, that I am afraid it will in some measure prevent your exaltation to honour. Dr. Simson has restored Euclid's Porisms. They are printed off, and will shortly be published. This intelligence I have from the best authority, viz., the Earl of Stanhope, at whose expense they are printed." Mr. Wildbore replied to this, "August 18th, 1775," and his letter contains the following passages:—"I much rejoice to hear that you have made an acquaintance with that learned and judicious nobleman, the Earl of Stanhope; and his communicating to you the contents of Dr. Simson's posthumous works is the reason of my being in such haste to answer your last agreeable favour. In contemplating what these Porisms of Euclid might be, and to what uses they were applied, I have hit upon something of which I have not met with the least hint in any author What I mean is this: *that the Porisms quoted by Diophantus, and the other remarkable Propositions, assumed without proof by that author,* which do so much honour to Antiquity, *are no other than the Porisms of Euclid,* and that this kind of Porisms composed Euclid's *second* Book But, as this may require something more than bare assertion, I shall here show the method by which some of the most remarkable flow from the two Lemmata* which are the 148th and 149th Propositions of the *seventh* Book of Pappus. From the first-named Lemma the following is evident. *In any right-angled triangle the double rectangle contained under the sum of the hypothenuse and greater leg, and the difference of the hypothenuse and cathetus* [the perpendicular] *is equal to the square of the sum of the said difference and the greater leg.*" He then deduces "the method which Diophantus makes use of in the 22nd Propo-

* 1. If upon a right line AB there be taken two points C, D, such that $2AB \cdot CD = CB^2$; then $AD^2 = AC^2 + BD^2$.—(*Pappus*, prop. 148.)

2. If $BA \cdot BC = BD^2$; then (*a*), $(AD + DC)BD = DA \cdot DC$;
(*b*), $(AD + DC)CB = DC^2$; (*c*), $(AD + DC)AB = AD^2$.
(*Pappus*, prop. 149.)

sition of his third Book, which many of the moderns have been very illustrious about, but for elegance am afraid they must all yield to the ancients." Another corollary gives the "property made use of by Diophantus, in Question 24 Book V.;" and by generalising his method he deduces "the Porism quoted by Diophantus in the *third question of the fifth Book of Arithmetic.*"

Mr. Wildbore now takes up the second of the two Lemmas previously noticed, and, by an elegant and somewhat elaborate process, he deduces "*the very remarkable Porism* quoted by Diophantus* as the 5th question of the Fifth Book." He further adds that—"there are many other curious properties flowing from the squares here deduced. Thus in the second square, if DC=1, we have question 33, Book II., of Diophantus; and if there be *any* number which is equal to the product of two numbers differing by unity; and if to this number unity be added, the square of the sum may be thus divided into *three* squares. Hence the sum of three squares may be easily divided into three other squares."

Towards the close of the letter Mr. Wildbore gives a *geometrical* solution to the following case of the general *Problem of Inclinations.* "*between two given semicircles, having their bases in the same right line, to insert a right line of a given length which shall verge to an angle of one of the semicircles.*" He says, "it is as curious as any we know that were done by the ancients, and having some time since bestowed pains to solve it without Dr. Horsley's *algebraical* Lemmas, I will here subjoin the solution." He concludes by stating that should Mr. Lawson "communicate the preceding to his lordship, as I am in hopes you will, please to make my most respectful compliments to him, and tell him that I shall wait with patience till I can be favoured with a sight of what Dr. Simson has done."

* "Etant donnés deux nombres carrés cousécutifs, on peut trouver un troisième nombre ègal au double de la somme de ces deux premiers plus 2, tel, que le produit de deux de ces nombres augmenté, soit de la somme des deux mêmes, soit du troisième nombre, fasse un carré."—Chasles, *Porismes*, p. 49.)

If x, y, z be the numbers—then $xy+(x+y)$; $xz+(x+z)$; $yz(y+z)$; $x+yz$; $y+xz$; $z+xy$,—are all to be squares under the conditions proposed. Quest. III. requires that $x+a$; $y+a$; and also $xy+a$, be square numbers.

It thus appears that the Rev. Charles Wildbore had discovered that these properties of numbers partook of the nature of Porisms more than sixty years before M. Chasles announced the same fact in his *Aperçu*. I am of opinion that the rev. gentleman was somewhat in error as to the contents of Euclid's Second Book; and his opinions on some other points mooted in this long letter will now require modification. There, however, appears to be sufficient merit in this correspondence to warrant the publication of these extracts, at a time when M. Michel Chasles is astonishing the scientific world by his connection with the Newton and Pascal forgeries.

"On the Examination of Water for Organic Matter," by Dr. R. Angus Smith, F.R.S.

The Author repeated his opinion that the mere expression of organic matter had no such meaning as would allow chemists to measure the impurity of water by its amount. He went more fully into the division of the organic matter into various portions, some acting as unwholesome agents, others being entirely innocent. He said he was glad to find that other chemists were also attending to the quality as well as the quantity of the organic matter, and he insisted also on the condition of the matter being observed. He discussed the methods of Professors Frankland and Wanklyn, considered, however, that they did not supersede his own methods, which made a greater number of subdivisions. He explained the mode in which the organic matter is entirely removed from water, leaving frequently more of its elements behind, unless we include amongst them the inorganic bodies with which they were combined. The body which remains is chiefly common salt, which cannot be removed, and by which more than any other substance animal matter is to be detected in water under certain precautions. He also showed the importance of finding the amount of atmospheric oxygen in water, and its meaning; but as the paper was not concluded the notice is here left incomplete.

Ordinary Meeting, January 7th, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"Variable Spot on the Moon's Surface," by W. R. BIRT, F.R.A.S., communicated by J. BAXENDELL, F.R.A.S.

The interest attaching to the phenomena presented by the lunar spot Linné is my apology for communicating a few observations on another spot which exhibits similar phenomena. It will be seen that both spots manifest phenomena which appear to be referrable to the presence of a *covering* by which the craters are at times *concealed*. We are not cognisant of any agency such as libration, angle of illumination, or variation of distance which affects the forms and appearances of lunar objects, being capable of rendering a crater *invisible* while its place is occupied by a white *cloud-like* spot of light; nor are any of these agencies capable of rendering an object on the moon's surface *indistinct* while others in its immediate neighbourhood are exceedingly sharp and well defined. With the hope of directing the attention of astronomers to this curious class of lunar objects, may I be permitted to lay the following observations before the Society? They have been made principally by the Rev. W. O. Williams of Pwllheli, who has undertaken the examination of a zone on the moon's surface of 2° of latitude, viz. from 4° to 6° south.

The spot in question is marked IV A*a* 17, IV Aζ 39 on the areas of the British Association Lunar Map IV A*a* and IV Aζ, and is situated in 2° W. long., and 5° S. lat. It is also situated on the S.W. side of the ridge forming the N.E. boundary of Hipparchus, and has been described as "a bright spot S.S.W. of IV A*a* 7 (Beer and Mädler's Hipparchus F). Its diameter is 5″·94 and magnitude 0°·37, the diameter of Dionysius being regarded as unity. On De La Rue's photograph 1858, February 22, it appears as a spot of about 4° of brightness. It is not so bright as Linné, which is about 5°. In this photograph it is seen to stand upon the east edge of a large *depression* running nearly S. by W.—N. by E. This edge, which forms a low ridge, connects the mountainous boundary of Hipparchus with the mountain IV Aζ 37. IV A*a* 7, a bright spot smaller than IV A*a* 17, IV Aζ 39, stands upon the west edge of this depression, which also meets the mountain IV Aζ 37.

On Rutherford's photograph 1865, March 6, this spot appears brighter than in De La Rue's, viz. 5°. Linné in this photograph is 6°. The observations that have been made of this spot are as under,

Year.	Date.	Authority.	Character.	Brightness.
1858	Feb. 22	De La Rue, Ph.	A bright spot.	4°
1865	Mar. 6	Rutherford, Ph.	A bright spot.	5°
1867	May 11 8½.....	Birt, Obs.	A shallow crater.	
1867	Oct. 7 8½ to 10.	Williams. ,,	A very bright spot.	
1867	,, 17	Ingall, ,,	A faint shallow crater.	
1867	,, 17 13½	Ingall, ,,	Drawn as a crater.	
1867	,, 17 13 to 15.	Williams, ,,	A very conspicuous crater.*	
1867	,, 18 17 to 19.	Williams, ,,	Crater very conspicuous, with a small central cone casting a shadow.	
1867	Nov. 5 9 to 10.	Williams, ,,	Very bright, a streak of interior shadow on the west.	7°
1867	,, 6 8 to 10.	Williams, ,,	A bright patch of light, streak of shadow scarcely discernible.	6°
1867	,, 15 18 to 20.	Williams, ,,	Very bright.*	10°
1867	Dec. 5 6 to 8.	Williams, ,,	A whitish spot, no trace of a crater.	5°
1867	,, 6 9 to 10.	Williams, ,,	A whitish spot, no crater.	5°

* On these occasions Mr. Williams saw a small bright point to the E., which he considered to be the *highest* point of the ridge.

Mr. Baxendell stated that on the night of the 3rd instant he had an opportunity of examining the spot referred to by Mr. Birt with Mr. Gladstone's equatorially mounted achromatic of $7\frac{1}{4}$ inches aperture, using powers from 60 to 250. It was then a well-marked though shallow crater, having a diameter about three-fourths of that of Beer and Mädler's Hipparchus F. The shadow of the western wall was very conspicuous on the floor of the crater.

Mr. Baxendell also read the following extract of a letter dated November 27th, 1867, which he had received from Mr. C. Ragoonatha Chary, the first native assistant at the Royal Observatory, Madras: —

"I have prepared the necessary calculations connected with the total solar eclipse to take place in the Indian Peninsula on the 18th of August, 1868, and these, with appropriate description and remarks on the eclipse by N. R. Pogson, Esq., are now in the press and will be published in the leading Madras Almanac. In these calculations I find that a slide-rule constructed for trigonometrical purposes may most advantageously be used even in such intricate cases as the solar eclipse. It saves more than three-fourths of the time and labour; and having calculated independently with the slide-rule as well as by means of logarithms for several places, I found the difference rarely to amount to half a minute in time, which is no great matter in predicting for amateurs, and even for intending observers. Mr. Woolhouse's method is followed, I believe, in the Nautical Almanac. The skeleton forms of this method, which are printed in great detail for logarithmic calculations, may be greatly simplified and facilitated by the use of a slide-rule accurately divided. The one I used was not very accurately divided, and was only two feet in length.

Mr. E. W. Binney, F.R.S., F.G.S., said that in the *Geological Magazine* for August of last year Mr. D. Mackintosh, F.G.S., in a paper on Pholas Borings, Denudation, and Deposition in S.E. Devon, says, "The structure and marine denudation of the district between Torbay and Babbicombe Bay has been so ably unravelled by Mr. Pengelly as to leave any other observer comparatively little to say. Among the most important discoveries must be ranked that of lithodomous perforations in limestone rocks at considerable altitudes above the sea." In the September number of the same publication the Rev. William Gibbins describes some Pholas borings and says, "And here I ought to mention that Mr. Pengelly was the first person from whom I learnt the supposed origin of these peculiar marks or holes in rocks near the sea coast." At a meeting of this Society on the 15th October last Mr. R. D. Darbishire, B.A., F.G.S., in a communication described similar holes and markings observed in the rocks near Buxton and on the Great Orme's Head.

No doubt it has been generally considered by geologists that Mr. Pengelly, F.R.S., a most acute and accurate observer, was the first person to notice the traces of lithodomous mollusks on rock surfaces. But it is now clear that such appearances had been observed and given to the public a quarter of a century ago.

In a Sketch of Furness and Cartmel by Charles P. Jopling, published in 1843, in speaking of the geology of Stainton and Adgarley at page 40, the author says, "The beds of the Carboniferous Limestone are well developed and are sometimes of great thickness.....On the green lie detached masses of rock as if formed

> Into rude shape by fire with roaring blast
> Impetuously let loose from central caves;
> Or fashioned by the turbulence of waves,
> That when o'er highest hills the deluge pass'd.

That at some time they have been submerged by the waters of the ocean seems indubitable, for the cavities in which the lithophagæ burrowed are still fresh, and tell of other aspects and other times."

At page 77, the author again says, in describing the country:—"We will not pretend to trace it through the many changes, or attempt to describe it when the breakers laved the rock of Stainton and the lithophagæ burrowed in those immense blocks which are still left to attest the fact."

The Carboniferous Limestone alluded to by Mr. Jopling in Furness, Lancashire, is only a few miles from the shore of Morecambe Bay, and in the localities alluded to by him will be at an altitude of about two hundred feet above the present sea-level.

He was not competent to form an opinion as to the holes and hollows in rocks seen and described by the above-named observers, but probably such phenomena, even if clearly proved to have been the work of a mollusk, should be classed under three heads, namely:—1st, those in the bare rock; 2nd, those covered by sward and vegetable mould; and, 3rd, those covered by till or other drift. In all these cases the shape of the original hole or hollow made by the Pholas would no doubt be considerably modified by the different causes to which they had been subjected. In the first case, they would be subject to the action of the carbonic acid in the rain-water, and aqueous and atmospheric agencies. In the second case, not only to those effects, but to the decomposing action of organic acids arising from the decaying vegetable matter. And, in the last instance, to long-continued aqueous and atmospheric agencies before the rock was covered up by the drift deposits.

Great care ought to be taken before a hole in a rock should be attributed to a mollusk, especially in limestones, which so frequently exhibit on the surface markings of nearly every variety of form.

Dr. JOULE, F.R.S., referring to recent magnetic disturbances, described the means he made use of for determining the absolute horizontal intensity. A current of electricity was transmitted successively through a single-coil galvanometer and his current meter, an instrument already described, consisting of a suspended flat coil of wire between two equal fixed coils. If the coils are infinitely close to one another, the current is given by the formula

$$C=\frac{k}{2l}\sqrt{gw}.$$

k being the radius of a circle of equal area to one of the coils, l the length of wire in one of the coils, and w the weight required to balance the suspended coil. The current in the galvanometer is determined by the horizontal intensity and the tangent of deflection. Therefore, by combining the two instruments, we obtain the horizontal intensity, which is equal to the square root of the force of attraction in the current meter, divided by the tangent of deflection in the galvanometer, multiplied by a constant.

"On the Examination of Water for Organic Matter," Part II., by Dr. R. ANGUS SMITH, F.R.S.

At present the conclusion only is given, as no abstract was prepared.

The following may be considered as a summary of the results required for sanitary purposes.

1. Quality of the organic matter, *i.e.* what is produced by standing under favourable circumstances for developing vegetable or other life?

2 and 3. Condition of the organic matter. Products of decomposition. Easily decomposed organic matter. These two can be estimated for sanitary purposes sufficiently by permanganate of potash.

4. Nitrates as remnants of organic matter.

5. Nitrites as remnants of organic matter.

6. Chlorides as indicating animal sources.

7. Oxygen as indicating activity of decomposition or destruction.

8. Total organic matter and ammonia, by weighing and other methods.

PHYSICAL AND MATHEMATICAL SECTION.

December 31st, 1867.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"Solar Radiation Observations, made at Eccles, near Manchester," by THOMAS MACKERETH, F.R.A.S., F.M.S.

Feeling considerable interest in the paper read by Mr. Baxendell, F.R.A.S., at a recent meeting of the Physical Section, and at a general meeting of the Society, on this subject, I have reduced five years' observations which I had made at Eccles with an ordinary black-bulb thermometer placed on the south side of the shade stand, four feet above the ground. The thermometer had been duly compared at the Kew Observatory. I was chiefly induced to make these reductions because the discussions of Mr. Baxendell on the subject were from observations made at Oxford by means of a thermometer placed somewhat similarly to mine, at least as mine was placed during the five years I have reduced.

Taking the differences between the mean maximum black-bulb reading in the sun, and the mean maximum reading in the shade, for each month, we find two maxima—one in May, and another in August; and two minima—one in June, and another in December.

In order to project the gradual rise and fall of solar radiation for each month, I have adopted the form of table presented by Mr. Vernon in his reductions of similar observations. Thus, if we take the differences between one month and the succeeding one, we find the following values:—

	°
January to February	+ 2·80
February to March	+ 2·98
March to April	+ 3·16
April to May	+ 1·14
May to June	− 0·80
June to July	+ 0·38
July to August	+ 0·56
August to September	− 1·48
September to October	− 5·28
October to November	− 2·26
November to December	− 3·06
December to January	+ 1·86

The foregoing table shows a very regular though rapid increase of the amount of solar radiation from January till April, which is only slightly exceeded in May. We have then a slight decrease, and afterwards another maximum in August. Then follows a very rapid decrease to October, which continues, though not so rapidly, to the end of the year. This table shows that the observations at Eccles have a projection very similar to the one presented by Mr. Baxendell on page 38, vol. VII. of the "Proceedings;" and I have no doubt that if the observations had extended over a longer period, the similarity would have been more striking.

Below I present my monthly means as they have been reduced.

Year.	January.			February.			March.		
	Max. Bk Bulb in Sun.	Max. in Shade.	Diff.	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.	Max. Bk. Bulb in Sun.	Max. in Shade.	Diff.
	°	°	°	°	°	°	°	°	°
1862	48·0	42·5	5·5	52·7	45·4	7·3	55·1	46·7	8·4
1863	49·8	44·3	5·5	55·5	47·4	8·1	62·4	49·9	12·5
1864	44·9	40·0	4·9	50·5	40·4	10·1	57·8	45·6	12·2
1865	44·6	40·3	4·3	44·8	40·0	4·8	51·8	42·1	9·7
1866	49·9	46·7	3·2	52·1	45·0	7·1	54·4	44·9	9·5
Means	47·44	42·76	4.68	51·12	43·64	7·48	56·30	45·84	10·46
	April.			May.			June.		
1862	66·9	53·3	13·6	77·5	61·6	15·9	73·2	60·8	12·4
1863	68·6	53.5	15·1	72·8	57·3	15·5	80·5	63·9	16·6
1864	69·1	54·9	14·2	80·8	62·4	18·4	78·8	62·6	16·2
1865	73·8	60·3	13·5	71·9	60·5	11·4	79·8	67·4	12·4
1866	65·4	53·7	11·7	69·8	57·2	12·6	79·0	66·8	12·2
Means	68·76	55·14	13·62	74·56	59·80	14·76	78·26	64·30	13·96
	July.			August.			September.		
1862	78·0	63·4	14·6	79·1	64·5	14·6	75·0	60·7	14·3
1863	82·4	66·7	15·7	82·2	65·4	16·8	69·8	56·9	12·9
1864	84·1	67·8	16·3	79·5	64·4	15·1	76·3	61·8	14·5
1865	81·9	68·8	13·1	78·1	64·7	13·4	85·3	70·2	15·1
1866	78·0	66·0	12·0	78·4	63·8	14·6	69·8	59·5	10·3
Means	80.88	66·54	14·34	79·46	64·56	14·90	75·24	61·82	13·42
	October.			November.			December.		
1862	65·3	54·7	10·6	51·8	42·4	9·4	51·8	47·5	4·3
1863	62·4	53·5	8·9	55·4	49·1	6·3	51·5	47·7	3·8
1864	62·2	54·2	8·0	53·1	47·4	5·7	45·0	42·6	2·4
1865	65·9	56·3	9·6	55·5	49·3	6·2	50·4	47·2	3·2
1866	60·9	57·3	3·6	51·5	49·7	1·8	47·5	47·1	0·4
Means	63·34	55·20	8·14	53·46	47·58	5·88	49·24	46·42	2·82

MEAN MONTHLY VALUES.

	Max. in Sun.	Max. in Shade.	Diff.
	°	°	°
January	47·44	42·76	4·68
February	51·12	43·64	7·48
March	56·30	45·84	10·46
April	68·76	55·14	13·62
May	74·56	59·80	14·76
June	78·26	64.30	13·96

	Max. in Sun.	Max. in Shade.	Diff.
	°	°	°
July	80·88	66·54	14·34
August	79·46	64·56	14·90
September	75·24	61·82	13·42
October	63·34	55·20	8·14
November	53·46	47·58	5·88
December	49·24	46·42	2·82

Ordinary Meeting, January 21st, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"Notes on some of the Rarer Plants found near Llandudno," by JOSEPH SIDEBOTHAM, Esq.

North Wales is celebrated for its botanical treasures, and perhaps no portion of it more so than the Great Ormes Head and the country adjoining. In the early spring Gloddaeth Woods are one mass of flowers, some of them by no means common, and the rocks and slopes of the hills afford many of extreme rarity. The cowslip, a rare plant near Manchester, is in abundance, and the hybrid oxlip is in many places where primroses and cowslips are found near together. Cowslips can be seen for more than a month, from the first of May, on the southern slopes and warm fields, till near the middle of June, on the northern slopes of the Little Ormes Head.

I will now mention a few of the rarer species met with during last spring.

CRUCIFERÆ.—*Malcomia maritima*, a species not truly wild, is however met with often on the Great Ormes Head. I have found it in many places all along the cliff walks and near the shore on Conway Bay. It is usually very small, the whole plant with flower not being more than a couple of inches in height.

Cheiranthus cheiri is found on most of the old castles and ruins in North Wales, and has evidently been there for a long time, being mentioned by various old writers. It is singular that it should so rarely be found on rocks, except in the immediate neighbourhood of some ruin. This would perhaps favour the idea that the species has been introduced at some very distant time.

Thlaspi alpestre. This is very plentiful at Llanrwst, the walls and rocks in many parts, being white with its flowers. This is the subspecies *occitanum* of *English Botany*; the other subspecies, *sylvestre*, I have not gathered, but from the examination of dried specimens of it from Teesdale and Clova, I cannot see a good reason for any division of the species.

Brassica oleracea. Found abundantly on the cliffs of the Great Ormes Head, and also on rocks in several places between Conway and Llandudno. It is of special interest as being the parent of the numerous varieties of cabbage, cauliflower, and brocoli, and is, I think, truly indigenous in these localities. I can myself speak to its abundance on the Great Ormes Head thirty years ago, when Llandudno consisted of a very few fishermen's cottages, and I am told it is mentioned as being there more than a hundred years ago.

Helianthemum canum. This is one of the most conspicuous plants on the slopes of the Great Ormes Head, in the early part of May, covering the ground with a beautiful pale yellow; about the last week in May it gives place to the other species, *H. vulgare*, the larger flowers of which make even a more beautiful show.

Helianthemum guttatum. This occurs on Holyhead mountain. I had previously gathered specimens in seed, and visited the place in May to see it in flower. I was a little early and only met with a few flowers fully out, and as the petals fall at once when touched, it was not easy to

secure good specimens. I however brought a few plants in my case, and planted them in a pot, where they flowered outside my window at Llandudno. On the bleak slopes at Holyhead the entire plant in flower does not exceed 1½ inches in height, but in the flowerpot it reached 3 to 4 inches. I made a drawing of one of the specimens, which can be compared with the dried ones. The red spot at the base of the petals is very large; the figure in *English Botany* does not do it justice.

Viola hirta. Found in many places about Llandudno and Conway, abundant in Gloddaeth woods.

Silene nutans. Abundant on the rocks about Llandudno.

Astragalus glycyphyllus. This is by no means a common plant. It is found in various places about Gloddaeth, in the lanes and also in the wood, and in one or two places near the Little Ormes Head. I was too early in May to find it in flower.

Hippocrepis comosa. A very handsome plant, on the rocks of the Great Ormes Head, where it forms large masses of flowers in May.

Cotoneaster vulgaris. The Great Ormes Head is supposed to be the only locality for this species in Great Britain. I visited Llandudno in the year 1840 chiefly to gather it, and in the same place I again collected specimens in May, 1867. I know of five distinct places where it grows pretty plentifully, and, if not rooted out, it will continue for generations to gratify botanists and supply them with specimens. But I should like here, to protest against the destruction of wild plants, which, if continued as at present, would in a few years entirely eradicate all the rare species, and make some of those now common, very rare. This destruction is not caused by botanists: a true botanist is careful not to destroy a plant, and in case of a very rare one, will content himself with a small sprig, without disturbing the root.

Even a collector of plants who takes no such care, will not do much harm in a general way, because he will only take as many specimens of a plant as he can dry. The real destroyers of plants are those who collect them for the market for sale. In Llandudno, for instance, the market is supplied with ferns and rare plants in prodigious numbers: these are purchased by visitors, as just now it is the fashion to take roots of wild plants on returning home. It is clearly the interest of those persons who supply the markets to clear the neighbourhood of rare plants, so that the visitor who wants them must buy them in the market. A very few years ago *Asplenum adiantum-nigrum* was one of the very common ferns around Llandudno; last May I wished to show it to a friend, and could not find a single frond nearer than a lane at Gloddaeth. One day in May I saw in the market almost a cart load of roots of *Allosurus crispus*, and, what was more to be regretted, a number of roots of *Cotoneaster*. As long as these dealers find purchasers they will continue to root up the rare plants; but if visitors would only consider that the great charm of our wild plants is to see them growing in their native places, and not to take away roots purchased in the market, which will rarely live when they are removed, this destruction would soon cease, and the natural yearly increase of the plants would be sufficient to supply those who would go and gather for themselves.

The neighbourhood of Llandudno is very rich in Compositæ, but I was too early in May to find many of them in flower.

Silybum Marianum is found in many places, and seems quite at home near the summit of the Little Ormes Head.

Serratula tinctoria, Cichorium Intybus, Helmintha echioides, Hypochœris maculata, are conspicuous along with several of the rarer species of *Hieracium*. Perhaps the rarest of the Compositæ is

Chrysocoma Linosyris, which is found abundantly on the rocks above the town. The specimens now exhibited were gathered late in August, when it was in perfection. This is one of the very few places in the kingdom where it is to be found.

Scrophularia vernalis. This is one of the rarest of British plants, and is supposed by some to be a doubtful native. However this may be, it is quite naturalised in a lane near Gloddaeth, where it ornaments the hedge sides for half a mile; it is also found in another lane nearer Conway. It is an exceedingly beautiful plant, with its pale green leaves and lemon coloured flowers. I found on this plant three species of Coleoptera of the genus *Cionus*.

In the ditches on the plain grows *Hottonia palustris* in the greatest perfection, the leaves in many cases growing in dense tufts above the water, and the beautiful spikes of flowers in thousands.

On the southern slopes of the Great Ormes Head, *Scilla verna* begins to show its bright blue flowers the first week in May, and in the middle of the month many places are completely covered with blue, and it may still be found in abundance in June on the northern and eastern sides of the hill; it even occurs in profusion down on the grass above the sea, and in the open places in Gloddaeth woods, where it grows to the height of six or eight inches.

The genus *Statice* is one about which botanists are not agreed as to the species, some authorities making many into species which others call only varieties. At Llandudno are found two very different forms of the species now called *binervosa;* the smaller form grows in abundance on Conway beach, and the larger one on rocks behind the baths. Although so different in appearance, there is no good ground for considering them in any way distinct species.

Among the grasses, *Phleum arenarium* and *Sclerochloa rigida* and *loliacea* are very common on and near the shore at Llandudno. The rarest species is *Knappia agrostidea*, found in Anglesey on the borders of Llyn-Curon, which is the only known locality for it in Great Britain. It is a very small species, and, flowering as early as from the end of March to May, it may have been overlooked in other places; it is extremely abundant on the grassy hillocks on the borders of the lake; the largest specimens I found were about two inches in height, the general size being about one inch.

In the foregoing notes I have only mentioned a few of the plants I met with in North Wales last spring. Specimens of many others are before you, and those who visit Llandudno a month later than I did will find very many more species, especially in the families *Compositæ* and *Cyperaceæ*.

"Some Remarks on Crystals containing Fluid," by J. B. Dancer, F.R.A.S.

The Author gave a brief history of the discovery of fluids in crystals, including Sir H. Davy's chemical experiments on the fluids and gases obtained from the cavities in quartz crystals; Sir David Brewster's discovery of the pressure cavities in the diamond, ruby, emerald, amethyst, chrysoberyl, &c.; the existence of minute crystals in these cavities and the two new and remarkable fluids, which are immiscible, but sometimes found together in the same cavity—one a liquid hydro-carbon, named Brewstoline, the other Cryptoline; his experiments and examination of artificial crystals

deposited from aqueous solutions; his examination of the Koh-i-noor diamond and others in the East India Company's museum; and the geological speculations to which these discoveries gave rise. Mr. Dancer mentioned the experiments of his late father and others in producing artificial gems by intense heat, and stated that his own attention was drawn to this subject some twenty-four years since, by Sir David Brewster presenting him with a specimen of topaz containing fluid. Since that time he had examined a large number of crystals, of various kinds, from the collections of friends; and had found fluid in quartz from South America, Norway, the Alps, Ireland, Snowdon, and the Isle of Man; and in fluor spar from Derbyshire; this latter specimen contained a considerable quantity of fluid, which burst the crystal at 180° temperature.* He suggested the employment of the microscope as a valuable assistance in detecting spurious from real gems; very few of the latter are perfect, and the flaws and cavities are so distinct in character from those which are so abundant generally in artificial gems that very little experience is sufficient for the purpose. This mode of testing of course is limited to transparent crystals, but might be employed when the usual methods are not practicable. He also mentioned Mr. Sorby's (F.R.S.) discovery of fluid cavities in the quartz of granite, in the quartz of volcanic rocks, and also in the feldspar ejected from the crater of Vesuvius, and Mr. Sorby's method of determining the temperature at which various rocks and minerals are

* After this Paper was written, Sir David Brewster informed the Author that the fluid contained in crystals of fluor spar was water, and that the cavities burst at a temperature of 150°.

formed. At the conclusion of the meeting, crystals containing fluid were exhibited under the microscope, and the expansion of the fluid by elevating the temperature of the crystal whilst under examination.

Ordinary Meeting, February 4th, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Among the donations announced were several bottles of Chemical Products for the Society's collection, from Dr. F. Crace Calvert, F.R.S., &c.

The thanks of the Society were voted to Dr. Calvert for his valuable donation.

"On Some Constituents of Cotton Fibre," by E. SCHUNCK, Ph.D., F.R.S., &c., President.

It is generally supposed that cotton, when quite pure, consists entirely of woody fibre or cellulose, and that its composition is consequently represented by the formula $C_{12} H_{10} O_{10}$. It is certain, however, that in the raw state, as furnished by commerce, it contains a number of other ingredients, some of which occur so constantly that they may be considered essential constituents of cotton, viewed as a vegetable product. The object of the bleaching process to which most cotton fabrics are subjected is to deprive the fibre of these other ingredients and leave the cellulose behind in a state of purity. Notwithstanding the importance of an accurate knowledge of everything relating to cotton from an industrial point of view, the substances contained in it along with cellulose have never been subjected to a special chemical examination, and very little is consequently known about them. Persoz, in his *Traité de l'Impression des Tissus*,

says that the woody fibre constituting the tissues of cotton, hemp, linen, &c., is not pure; it contains 1st, a certain quantity of colouring matter, which is more or less shielded from the action of decolorising agents by the bodies which accompany it, naturally or accidentally; 2ndly, a peculiar resin natural to the fibre, insoluble in water and soluble with difficulty in alkalies, which plays the part of a reserve and protects the colouring matters inherent in the fibre from the action of the agents which ought to destroy and remove them; 3rdly, a certain quantity of fatty matter, of which a very small portion is peculiar to the fibre, the greatest part being derived from the operations of spinning and weaving; 4thly, a neutral substance, either flour, starch, or glue, which has been introduced by the weaver in sizing his warp; 5thly, inorganic saline matters, some of which belong to the fibre, while the others are derived from the water and the matters employed in the dressing of the warp. In the excellent article on Bleaching in the new edition of Ure's *Dictionary of Arts* there is a full account of these and other impurities of cotton fabrics, comprising all that was known at the time when the author commenced his examination.

The object which the author had in view in undertaking his investigation was to endeavour to throw a little more light on the nature of those substances which are contained in or attached to the framework of cellulose, of which cotton fibre mainly consists, and which are together with the latter produced by the plant. All foreign and extraneous matter introduced during the process of manufacture was therefore left entirely out of consideration. The author has further confined his attention to those constituents of the fibre which are insoluble in water but soluble in alkaline lye, and are afterwards precipitated by acid from the alkaline solution. Whether cotton contains naturally any substance soluble in water, or which being originally insoluble is rendered soluble therein by the prolonged

action of alkalies is a question on which the author pronounces no decided opinion.

For the purpose of obtaining the substances which he proposed to examine the author employed cotton yarn, which he preferred to unspun cotton for several reasons, the principal being that yarn is comparatively free from mechanical impurities, such as fragments of seed-vessels, &c., while on the other hand, if proper care be taken, no impurity is added to those previously existing during the process of spinning. The yarn was boiled in an ordinary bleacher's kier for several hours with a dilute solution of soda ash. The resulting dark brown liquor, after the yarn had been taken out, drained and slightly washed, was removed from the kier into appropriate vessels, and mixed with an excess of sulphuric acid, which produced a copious, light brown, flocculent precipitate, while the liquid became colourless. This precipitate was allowed to settle, the liquid was poured off, and after being washed with cold water to remove the sulphate of soda and excess of acid it was put on calico strainers and allowed to drain. A thick pulp was thus obtained, which when dried assumed the appearance of a brown, brittle, horn-like substance translucent at the edges. In one experiment 450 lbs. of yarn, made from East Indian cotton, of the variety called "Dhollerah," yielded 0·33 per cent. of the dried precipitate. In another experiment made with 500 lbs. of yarn, spun from American cotton, of the kind called in commerce "middling Orleans," 0·48 per cent. was obtained. The total loss sustained by yarn during the bleaching process amounts to about five per cent. of its weight. Only a small portion of the matter lost is therefore recovered by precipitation of the alkaline extract with acid.

This precipitate formed more especially the subject of the author's investigation. It was found to consist almost

entirely of organic substances, and of these the following were distinctly recognised :—

1. A species of vegetable wax.
2. A fatty acid.
3, 4. Colouring matters.
5. Pectic acid.
6. A trace of albuminous matter.

The author described the method employed by him for separating these substances from one another and obtaining them in a state of purity; and he then gave an account of their properties and composition.

The waxy matter is by far the most interesting of these substances. It is insoluble in water, but soluble in alcohol and ether. If a concentrated solution in boiling alcohol be allowed to cool, the greatest part is deposited, causing the liquid to assume the appearance of a thick white jelly, consisting of microscopic needles or scales. When this jelly is filtered off and dried it shrinks very much, and is converted into a coherent cake, which has a waxy lustre and is translucent, friable and lighter than water. Its melting point is between 83° and 84° C. At a higher temperature it is volatilised. When heated on platinum it burns with a very bright flame. The author thinks it probable that this substance covers the cotton fibres with a thin waxy film, and thus imparts to them their well-known property of resisting water. In its properties and composition it approaches very nearly the better known vegetable waxes, such as that obtained by Avequin from the leaves of the sugar cane, and that which is found on the leaves of the Carnauba palm. The author thinks that the name *cotton wax* is sufficient to distinguish it from these and other nearly allied bodies.

The fatty acid has the properties and composition of margaric acid. It is white and crystalline, fuses at 53° C.,

and gives with alkalies compounds soluble in water which are true soaps. It is, however, probably not a natural constituent of cotton fibre, but rather an impurity derived from the oil of the seed which escapes and diffuses itself among the cotton before or during the process of ginning. It might also have had its source in the oil and fat, used for greasing the cotton spinning machinery, since the author employed yarn in all his experiments. Persons practically conversant with cotton spinning affirm, however, that if ordinary care be taken, it is impossible that the cotton can become contaminated with anything of a fatty nature during its conversion into yarn.

The colouring matters obtained in these experiments are without doubt the substances to which raw cotton owes its yellowish or brownish colour. The author was able to distinguish two bodies of a dark brown colour, which occurred in all kinds of cotton examined by him. Of these one is easily soluble in cold alcohol, and is left, on evaporation of the solution, as a dark brown, shining, brittle, amorphous resin, which is transparent in thin layers. In boiling water it softens and melts to a pasty mass, which becomes hard and brittle again on cooling. When heated on platinum foil it burns with a bright flame, leaving a very voluminous coal. It is nearly insoluble in ether. It dissolves easily in concentrated sulphuric acid and glacial acetic acid, with a brown colour. It also dissolves with ease in caustic and carbonated alkalies, giving dark yellowish-brown solutions, from which it is re-precipitated by acids in light brown flocks. The other colouring matter resembles this in most of its properties. It is, however, much less soluble in alcohol. Cold alcohol, indeed, dissolves only a trace, but in boiling alcohol it dissolves with tolerable facility, being re-deposited, on the solution cooling, in the form of a brown powder. This powder, when filtered off and dried, forms coherent masses

of a colour varying from light to dark brown, which are easily broken, showing a dull earthy fracture. Both colouring matters contain nitrogen, and they differ therefore in constitution from true resins, which they resemble in many of their properties. The peculiar colour of the so-called "Nankin cotton" is probably due to a great excess of these colouring matters existing in the fibre. It is certainly not caused by oxide of iron.

The purification of the pectic acid contained in the brown precipitate produced by sulphuric acid was not effected without difficulty. The best method, according to the author, consists in submitting it to a simple process of bleaching with chloride of lime, by which means the impurity, consisting of brown colouring matter, which adheres to it with great pertinacity, is destroyed. When pure it has the properties and composition ascribed to pectic acid by Fremy. The cotton itself probably contains pectose or pectine, which is converted into pectic acid by the action of the alkaline lye. About three-fifths of the brown precipitate consists of pectic acid. Of the remaining two-fifths the colouring matters constitute by far the largest part, the wax and fatty acid being present in very minute quantities. The albuminous matter was not isolated, but its presence was indicated by the formation of a small quantity of leucine, which took place when the brown precipitate was submitted to the action of hydrate of soda. A large quantity of oxalic acid was formed at the same time, no doubt from the pectic acid.

In conclusion, the author makes some remarks in regard to the part which these bodies may be supposed to play during the process of manufacturing gun cotton. It has been asserted that the instability occasionally observed in gun cotton is to be attributed to the impurities in the raw fibre, forming, by the action of nitro-sulphuric acid, bodies which decompose spontaneously at the ordinary or a

slightly elevated temperature. The author's experiments do not support this view, since the substances described by him, when submitted to the action of the mixed nitric and sulphuric acid of the strength employed for making gun cotton, do not yield explosive compounds.

PHYSICAL AND MATHEMATICAL SECTION.

January 28th, 1868.

JOSEPH BAXENDELL, F.R.A.S., Vice-President of the Section, in the Chair.

"On Solar Radiation," Part II., by JOSEPH BAXENDELL, F.R.A.S.

It has been shown in the first part of this paper that the curve laid down from the monthly means of solar radiation derived from the five highest values in each month, or those corresponding to the five clearest days, exhibits two maxima and two minima occurring respectively in April and September, and June and December. In order however to determine the influence of the seasons upon the amount of solar radiation on clear days, it will be necessary to correct these means for the difference of meridian altitude of the sun in the different months of the year. For this purpose I have employed the table given in the article on Climate in the *Encyclopædia Britannica*, referred to by Sir John Herschel in a note at foot of page 11 of his admirable *Treatise on Meteorology;* and I have neglected, as being immaterial for our present purpose, any correction which

might be necessary in a more exact inquiry in consequence of equal increments in the difference between sun and shade temperatures not corresponding exactly to equal increments of calorific intensity of the sun's rays. This table gives for every fifth degree of altitude the calorific intensity of sunlight, the intensity of the light on entering the earth's atmosphere being taken =1. If the rate of emission of heat from the sun were constant, and change of season exercised no influence on the absorptive power of the atmosphere, the quotient obtained by dividing the mean value of solar radiation on clear days in any month, by the number in this table corresponding to the sun's meridian altitude in the middle of the month, would be a constant quantity. The results given in the following table will however show that according to the Oxford observations this is by no means the case:—

	Mean Amount of Solar Radiation on Clear days.	Meridian Altitude of the Sun on the 15th of the Month.	Proportion of Light Transmitted.	Number in First Column divided by Number in Third Column.
	°	° ′		
January	13·5	17 6	·37	36·5
February	17·6	25 32	·51	34·5
March	17·8	36 3	·61	29·2
April	20·3	47 57	·68	29·8
May	19·2	57 4	·71	27·0
June	18·1	61 34	·72	25·1
July	19·2	59 50	·72	26·7
August	19·6	52 23	·69	28·4
September	20·0	41 22	·65	30·8
October	18·1	29 48	·56	32·3
November	15·6	19 48	·43	36·3
December	13·0	14 59	·33	39·4

The numbers in the last column show that the power of the atmosphere to absorb the heating rays of the sun is much greater in the summer than in the winter months, the maximum effect taking place in the month of June, when the

sun attains his greatest meridian altitude, and the minimum in December, when his meridian altitude is least. As the amount of aqueous vapour in the atmosphere is much greater in summer than in winter, this result tends strongly to confirm the view taken in my paper "On the Theory of Rain," and since held to be established by Professor Tyndall's experimental investigations, that air charged with aqueous vapour has a much greater power of absorbing and radiating heat than dry air. If however we use the Greenwich observations in this investigation we shall arrive at a totally different result. Taking, for instance, those made in 1857, and treating them by the method employed in discussing the Oxford observations, we have the results shown in the following table:—

	Mean Amount of Solar Radiation on Clear Days.	Meridian Altitude of the Sun on the 15th of the Month.	Proportion of Light Transmitted.	Number in First Column divided by Number in Third Column.
	°	° ′		
January	9·0	17 19	·38	23·6
February	16·6	25 42	·51	32·6
March	18·2	36 37	·62	29·3
April	23·8	48 29	·68	35·0
May	24·8	57 31	·71	34·9
June	26·9	61 52	·72	37·3
July	26·8	59 59	·72	37·2
August	24·8	52 25	·69	35·9
September	22·9	41 21	·65	35·2
October	20·2	29 47	·56	36·1
November	9·6	19 52	·43	22·4
December	3·9	15 12	·33	11·9

The numbers in the last column of this table are considerably greater in the summer than in the winter months, thus indicating a greatly reduced absorptive action of the atmosphere in the sun's heating rays in the warmest half of the year, when the quantity of aqueous vapour in the air attains

a maximum; a result directly opposed to that derived from the Oxford observations, and to the conclusions drawn by Professor Tyndall from his experiments. If, however, we examine the conditions under which the observations were made at Greenwich, we find that while the ordinary thermometers on the shade stand were placed with their bulbs about 4 feet from the ground, the solar radiation thermometer was placed in an open box about 13 inches high with its bulb about 10 inches above the bottom of the box. Now, a little consideration will show that on a clear calm day in summer the air in this box will be heated to a temperature several degrees above that of the air at 4 feet from the ground; while on the other hand in winter it will often be several degrees colder. The readings of a thermometer placed in it will therefore be too high in summer and too low in winter; and the magnitude of the errors may well be sufficient not only to mask the true action of the varying amount of the aqueous vapour in the atmosphere, but to lead to conclusions directly at variance with the truth.

The heating effect of the sun's rays at the surface of the earth during a given interval obviously depends upon the the greater or less prevalence of cloud or haze, and it is also evident that the amount of the latter will in general depend upon the degree of humidity of the air as determined from observations taken with the dry and wet bulb thermometers. Any alterations therefore which may take place in the calorific intensity of the sun's light ought to be indicated by a comparison of the differences between the mean temperatures of the air and of evaporation, and those between the maximum temperatures in the sun and in the shade. The values of

these elements for the years 1859–64 at Oxford, and their ratios, are as follows: —

	Maximum in Sun less Maximum in Shade.	Temperature of Air less Temperature of Evaporation.	Ratio.
1859	12·85	2·67	4·81
1860	10·78	1·98	5·44
1861	11·24	2·49	4·51
1862	9·37	2·72	3·44
1863	9·78	2·87	3·40
1864	9·81	2·73	3·59

A projection of the ratios gives a curve closely resembling the curve No I, diagram B (p. 39), and therefore strongly supporting the view that the calorific intensity of the sun's rays varies in a period corresponding with the solar spot period.

If instead of the difference between the mean tempertures of the air and of evaporation we employ the difference between the mean temperatures of the air and of the dew point we obtain the following ratios.

1859	2·33
1860	2·57
1861	2·17
1862	1·66
1863	1·65
1864	1·72

It will be seen that the curve representing these ratios is identical in form with that representing the last series of ratios, as indeed might have been expected.

Proceeding now to apply the same method to the treat-

ment of the mean monthly values of solar radiation we have the following table :—

	Mean Monthly Solar Radiation.	Mean Temperature of Air less Mean Temperature of Evaporation.	Ratio.
	°	°	
January	5·51	1·31	4·20
February	9·16	1·75	5·23
March	10·08	2·36	4·27
April	13·23	3·00	4·41
May	12·90	3·78	3·41
June	12·93	3·43	3·76
July	13·81	4·15	3·32
August	14·03	3·73	3·76
September	13·71	2·81	4·88
October	9·66	2·05	4·71
November	8·00	1·41	5·67
December	4·50	1·15	3·91

Before the true import of the numbers in the last column can be clearly understood, it will be necessary to apply corrections for the changes of meridian altitude of the sun in the different months of the year, and employing for this purpose the table in the article on climate in the *Encyclopædia Britannica* already referred to, we obtain the following corrected numbers :—

January	1·59	July	2·39
February	2·66	August	2·59
March	2·60	September	3·17
April	3·00	October	2·63
May	2·42	November	2·44
June	2·70	December	1·29

The highest values occur in April and September, the months in which the diurnal oscillations of the declination magnetometer are greatest, and the general course of the numbers indicates that clouds and haze are less prevalent, or less dense, or their power of absorbing the heating rays

of the sun less active, in the spring and autumn than in the winter and summer months; and in connection with this it may be remarked that the rate of change in the difference between the temperature of the air and the temperature of evaporation is greatest in the months of April and September.

Since the first part of this paper was printed in the Society's Proceedings, Mr. MACKERETH, F.R.A.S., has communicated to the Physical and Mathematical Section the monthly results of his Solar Radiation Observations made at Eccles during the five years 1862–66. As these observations were made with a black-bulb thermometer placed somewhat similarly to that used at Oxford, and as the series extend two years beyond the Oxford series, it was evidently very desirable to examine how far they confirmed the conclusions derived from the Oxford observations. The mean annual values of solar radiation having been deduced from the monthly means, and Mr. Mackereth having kindly supplied me with the annual mean temperatures of the air and of the dew-point, they were treated on the plan employed in discussing the Oxford observations. These data and the results thus derived from them are shown in the following table:—

	Mean Temperature of Air.	Mean Temperature of Dew Point.	Difference.	Mean Amount of Solar Radiation.	Ratio.
	°	°	°		
1862	47·3	42·3	5·0	10·90	2·18
1863	48·3	42·3	6·0	11·47	1·91
1864	47·0	40·5	6·5	11·50	1·77
1865	48·6	43·3	5·3	9·72	1·83
1866	48·3	42·3	6·0	8·25	1·37

The mean ratio for the two years 1865–6 is 1·60, while

that for the years 1862–4 amounts to 1·95. It appears therefore that the calorific intensity of the sun's rays continued to diminish for two years after the termination of the Oxford series, and as the observations of Schwabe, Wolf, Balfour Stewart, and others, have shown that the frequency of solar spots also diminished during these two years the probability that a close connexion exists between the two phenomena is considerably increased by the results of Mr. Mackereth's short but valuable series of observations.

On comparing the Oxford and the Eccles series of results it will be seen that the values for the years which are common to both (1862–4) are greater at Eccles than at Oxford in the ratio of 1·17 to 1·00, thus indicating that the blackened bulb of the thermometer at Eccles absorbs radiant heat more readily than that at Oxford. Dividing the Eccles mean annual values by 1·17 in order to reduce them to the Oxford scale and recalculating the ratios we have

	Reduced Values of Solar Radiation.	Ratios.
1862	9·33	1·82
1863	9·82	1·63
1864	9·84	1·51
1865	8·32	1·57
1866	7·06	1·17

Combining now the two series of ratios, and taking the means of the two values given for each of the three years 1862-4, we have finally, the following series of corrected ratios all referred to the same scale, and therefore strictly comparable with each other. I have added for comparison the

number of groups of solar spots observed in each year by Schwabe :—

	Ratios.	Number of Groups of Solar Spots.
1859	2·33	205
1860	2·57	211
1861	2·17	204
1862	1·74	160
1863	1·64	124
1864	1·61	130
1865	1·57	93
1866	1·17	45

The curves laid down from these two series of numbers present so close a resemblance in their general form, that it seems impossible to resist the conclusion that the causes which influence the frequency of solar spots also produce corresponding changes in the calorific intensity of the sun's rays.

In addition to the conclusions drawn from the discussion in the first part of this paper we have now the following :—

1. The power of the atmosphere to absorb the heating rays of the sun is much greater in the summer than in the winter months, and depends apparently upon the amount of aqueous vapour which it contains.

2. Clouds and haze are less prevalent during the day, or their power to intercept the heating rays of the sun is less active in the spring and autumn than in the winter and summer months.

3. Observations of solar radiation made with a black bulb thermometer, to be of value, ought to be taken with the radiation thermometer placed at the same height above the ground as the shaded maximum thermometer, with

which it is compared; but while freely exposed at all times to direct sun-light, it ought to be protected as much as possible from disturbing influences.

4. Solar radiation observations made on a plan similar to that adopted at Oxford, show that the calorific intensity of the sun's light continued to diminish during the years 1865-6, when the frequency of solar spots was also diminishing, thus giving additional weight to the probability that changes in the heating power of the sun's rays are intimately connected with variations in solar spot frequency.

Ordinary Meeting, February 18th, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

Dr. H. E. ROSCOE, F.R.S., communicated some of the results of his recent researches "On the Properties of Vanadium," and exhibited specimens of some of the more interesting of its compounds.

Ordinary Meeting, March 3rd, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On the Lunar Crater Linné," by Herr J. F. J. SCHMIDT, Director of the Observatory at Athens. Extract from a letter dated February 6th, 1868, addressed to Mr. Reimers, of Manchester. Communicated by J. B. DANCER, F.R.A.S.

"When I received your letter I had just returned from the Sartorin Islands. As regards the Linné crater, I find that it still presents the same appearance, viz., as a white cloud; but when the light is very sharp, I see more frequently than before a small opening or hole about one-tenth the diameter of the former crater.

"After the publication, by Baill of Leipsic, of my great Catalogue of the Ravines or Crevasses of the Moon, I discovered many other remarkable ones—an account of which I shall send soon to the Lunar Committee in London. The most complete account of my observations on Linné you

will find in my general report to the Academy of Vienna, published by Gerold.

"No eruption of a telluric nature can be the cause of a change in Linné, but a filling up of the crater by masses of mud or dust; otherwise the shadow of the smoke, as in an ordinary eruption such as I observed at Vesuvius and at the Sartorin Islands, would not have escaped my observation."

"Brief Notes on the Laws of Physical Force," by J. C. Dyer, Esq., V.P.

The wide acceptance of the new doctrine of the conservation of physical energy, or force, has led me to submit a few remarks thereon.

In the first place, let us keep in mind that there are five kinds of natural forces continually exerted by material bodies. Three of these are mechanical forces, and, as such, are measurable by the known laws of physics—viz., the force of gravity, of inertia, and of elasticity, each being co-extensive with, and inherent properties of all tangible bodies. The actual exertion of these, by their equal action and reaction, serves to sustain the positions and forms (whether in motion or at rest) of the entire material universe; and it is only by the observed action of such balanced forces that we are assured of the existence of material bodies.

Many theories have been propounded to account for the presence of these several forces in matter; yet no real light has been shed upon the questions involved in such inquiries beyond the fact that all these forces are found to be essential forces of the bodies exerting them. The two first are directly as the quantity of matter, and the third or elastic force varying in degrees of action according to the condition of bodies as solids, liquids, gases, or vapours. I have elsewhere aimed to show that a pervading "calorific

element" constitutes the essence, or is the source, of the elastic forces in all ponderable bodies — a question here passed by.

Besides the above mechanical forces, we have to consider the two other forces, namely, the chemical and vital forces. The affinities and repulsions of the component parts of matter, by which its mutations are affected, constitute the chemical forces. These forces are of varying and complex intensities, and are still but vaguely understood even by our most able practical chemists; yet the reality of such forces is extensively evidenced to our senses, though they are seldom such as can be distinctly measured like the mechanical forces.

With respect to the vital forces, they are of a still more complex and recondite nature, and we must be content in our present state of knowledge to place these forces among the many other sublime and mysterious laws impressed upon organic matter by an all-wise Providence, and which are not yet placed within the range of man's mental vision. We simply know that the vital forces are, in part, controlled by the will, and a greater portion of them are called into action by organic stimulants; and since both kinds of action cease with death, they are properly treated as vital forces. Whilst we know not how the vital forces control the mechanical and chemical forces exerted in and by living beings, there is no lack of proof that they do in fact command the other forces exerted through organic nature.

I shall here cite an eloquent passage from an essay (given in a popular journal, *Once a Week,* for 13th October, 1865) by one of the most able exponents of what is called the "New Philosophy," as follows, namely: —"The great philosophical doctrine of the present era of science, as the conservation of energy has been worthily styled—teaches us that the activity which we see manifested in all the natural forces is a constant quantity, or in other words, that there

is a definite amount of force distributed through nature, which is invariable in amount, and which we can neither add to nor take from, but whilst the force in one form disappears, it reappears in another form. But what do we mean by the term force? The simplest definition of the term is that which describes it as something which produces or resists motion. When we think of force we think of something moving or moved."

This definition of the term force would be unexceptionable, if what is called force, be held simply as indicating the properties of the bodies in which it acts, and not as a something, or energy, existing by itself, as an entity, apart from matter, but as a part of it. Were it merely intended to declare, that the sum of the mechanical forces, as gravity, inertia, and elasticity, is a constant quantity, just as is that of the bodies in which these forces are inherent properties, then there would be no grounds for asserting this fact, as disclosing a "great new doctrine;" for the indestructible nature of matter, and of its properties, were physical truths known and taught in our childhood.

The relations of matter and force—at least as regards the mechanical forces—have been set forth in such ample detail, comprising all known facts, in the elements of physical science, that it would seem needless to repeat the facts, of the unchangeable nature of the material universe, both as to the amount of, and the kind of forces, pertaining to matter, but for the enunciation of theories implying such changes, wherefore to treat of the conservation of the natural forces, and of the disappearance of "one kind of force and reappearance of another kind," must be held as employing unmeaning, or misleading terms. It is not that any of the forces themselves can disappear, or be changed from one to another kind of force; but that the motions generated by them may cease, or be rendered quite uniform when the moving forces are exactly balanced, by their action in opposite directions.

In short, the term force, used in an abstract sense, and without defining the kind of force intended, is worse than idle or unmeaning, because when so used by eminent physicists, it cannot fail to puzzle and mislead the younger students in the sciences. The author quoted frankly states that the dogmas he expounds—"seem not quite so clear to the unscientific mind."

Considering that this city is so widely distinguished, for its practical application of the mechanical and chemical sciences, it would reflect much discredit on the community, for us to be unable to comprehend the simple elements of the natural forces, so largely guided by our hands. Hence, a few words seem called for, to distinguish the several forces, acting conjointly, or separately; and to protest against their being confounded together, as an abstract entity to be conservated.

There seems no ground for asserting any new discoveries relating to the natural forces; yet a boundless field lies before us, for new applications of those forces, to the ever expanding and varying uses of man.

In addition to the before-mentioned fallacies, of including all physical forces under one head, and treating them as an abstract entity, it is assumed by the new philosophy, that vast mechanical forces, are continually exerted by special natural agencies, which hitherto have not been known to exert, or to possess this kind of force in any degree. To sustain this assumption, its authors have not been able to adduce any facts, or even analogies, in known phenomena, to prove or render probable the exertion of such forces, as, for instance, the alleged mechanical forces of the solar rays, when they are intercepted by aqueous vapour in the air, and the like forces assumed to be in continuous action, by and among the agitated atoms and molecules, or the ultimate particles of tangible bodies, and thus by their internal motions, tending to change, or to restore their sizes and

forms, when the bodies are altered in either by external forces. Now, considering that such ultimate particles, called "atoms" and "molecules" are not discoverable by any analysis of, or known mutations of matter, to assert the exertion of such invisible moving forces, as apart from the tangible matter exerting those forces, is clearly a gratuitous assumption, unwarranted by any known properties of material bodies. With respect to the solar rays, we have many striking evidences of their chemical action in terrestrial phenomena; but no proof whatever has yet been adduced, to show their mechanical force or action upon tangible bodies, unless their impinging on the optic nerves, giving the sensation of light, may be held to be a mechanical action; but even if this be so, the force exerted can be but slight. Wherefore these newly discovered forces must "vanish into air, into thin air."

From the same high authority (before named) after giving a brilliant exposition of "the source of the solar rays," or as it is termed, "the origin and sustentation of the heat of the solar furnace"—and showing how the growth of vegetables depends on the solar influence, and that our stores of coal come from plants, also that by the combustion of coal, steam-power is obtained and machinery driven, &c. &c. The author then adduces the following case, namely—

"From machine power, we turn to muscular power. Between the steam-engine and living bodies there is the closest analogy. The nutritive materials upon which life depends, are no more nor less than combustible substances, which actually undergo a slow combustion. The conversion of food into work done, is effected by the same process as that which turns coal, or wood into motive force."

That we must eat to live, and the steam-engine must have steam to work it, are simple facts that require no comments; but the forces exerted to sustain the movements

of a metallic engine, and the living organisms, are so widely different in the two cases, that one could hardly expect to see their close analogy, asserted by any writer on the laws of physics, except under strange illusions, concerning the kinds of forces employed in the economy of nature.

It is an obvious fallacy to assert such close analogy, to exist between the functional energies of living beings, and the force of steam in an engine. The distinction in the two cases, is plainly seen by reference to the two kinds of force, called into action by each of them; the first being a series of organic energies, that connect and sustain the necessive motions, through the complex structure of the said beings, the other being simply the elastic force that drives the engine. To support this alleged analogy, its authors should be enabled to bring some sort of proofs, that the vital forces do result solely, from the chemical forces evolved in the living system; but which is not sustained by any known facts, or deductions from facts. No "new philosophy" is required, to teach us that all organic beings, depend for their existence on solar heat, as also on the due supply of proper food. We also know that "the nutritive materials for sustaining life," contain a large portion of carbon, in the forms of gelatine, fibrine, saccharine, starch, adepose and other analagous bodies in common food; but these facts afford no explanation as to the origin, development and final decay of "nature's living progeny."

The vital forces, from the first germ to the close of life, exhibit in a clear plan, a train of connected and harmonious action, in guiding and controlling the chemical and mechanical forces, that are exerted in carrying on the functional powers of life—showing that the vital force was adapted to and designed to control the others.

The marvellous energy of the heart, far exceeds the force of any hydraulic engine of the like size; the forces exerted by the peristaltic, and other internal muscles, as also the

chemical forces acting through the lungs, stomach and general circulations are alike inexplicable, upon any theory however ingenious, of slow combustion, or of machine power.

Although the term vital force may be somewhat vague, it seems to point out the special energies, that originate organic forms, and to multiply and expand them by a series of spontaneous actions, from the seeds and ova, to build up the living structures of plants and animals, and upon the attainment of their maturity, the subsidence of those energies commences, nor can any intrinsic aid, or stimuli prevent their extinction at the appointed time.

If the term force be made to include those exerted by the vital organs, and if the conservation of such forces be a practical reality, then the living energies need never to "fade away and expire," except through the wilful neglect of the conservative doctors.

I will here add, in the words of a profound thinker, that, in organic life, "every train of development exhibits in its course an adherence to plan, which can only have its ground in an internal vital destination. It exhibits at the same time an independence of all external influences, which testifies to the internally given force of vitality."*

It must be observed that these comments on vital forces relate solely to those functional energies that produce physical action, and which are quite apart from the "moral and intellectual forces" exerted externally by sentient beings upon each other — a subject of wider and far higher nature than any of mere physical action.

I venture to recommend a careful perusal of the learned and eloquent essay (in *Once a Week*) before quoted, in order to a clear and full comprehension of what is called "the great philosophical doctrine of the present era of science."

All know that in the act of breathing, the carbon in the

* Ray Society's Bot. and Phys. Memoirs, 1863.

blood is converted into its acid form, whereby the heat of warm-blooded animals is mostly kept up. This process is properly enough termed combustion. It is alike known that in the act of digestion, all sorts of food taken into the stomach are decomposed and converted into one uniform fluid (chyle), which process bears no resemblance whatever to that of combustion; yet it is equally certain that animal life depends as strictly upon the digestion of food as upon healthful respiration. Indeed those chemical powers of the stomach are even more hidden and wonderful than the mechanical energy of the heart before noticed.

By the experiments of Dr. Beaumont, in the well-known case of the Canadian soldier's (St. André's) stomach, it was clearly proved that the many kinds of animal and vegetable food taken were dissolved and converted into a semi-fluid of one uniform substance in the course of about two to four hours, by the juice or fluid secreted by the epigastrium, the secretions following in succession and laying hold of the morsels of food as they were swallowed.

The secreted liquor (gastric juice) has never been formed in any other than "Nature's laboratory," nor has the chemist ever been able to discover the nature or constituents of this general solvent. How puerile, then, to talk of the analogy of these vital forces with those of slow combustion.

In conclusion let me add, that I make no pretence to any novelty in what is above said relating to the laws of force, but merely to have noticed some of the new principles and views that appear to be wholly untenable under every known law. To treat fully the many physical questions connected with the natural forces, and especially those of the vital forces, would demand (in place of a few pages) a work of ample size and labour, without exhausting the inquiries concerning obscure phenomena.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 6th, 1868.

J. B. DANCER, F.R.A.S., President of the Section, in the Chair.

Mr. SIDEBOTHAM exhibited a new revolving table for Microscopical purposes, made to his order by Mr. Rogerson; also a large Duck's egg, containing a second egg with a perfect shell.

Specimens of the following plants were offered for distribution among the members by Mr. NOTCUTT: Iberis amara, Hierochloë borealis, Caucalis daucoides.

Mr. DANCER read a Paper entitled "Some Remarks on Crystals containing Fluid."

Mr. SIDEBOTHAM also read a Paper on "Some of the Rarer Plants found near Llandudno."

[Both these Papers were afterwards read at the Ordinary Meeting of the Society, held January 21st, 1868, see pages 83—90.]

January 27th, 1868.

J. B. DANCER, F.R.A.S., President of the Section, in the Chair.

The PRESIDENT announced the death of J. Hepworth, Esq., Surgeon, who had occasionally contributed papers to

the Section. Mr. Hepworth's industry and success in the preparation of injected objects for the microscope, and his liberality in placing them at the disposal of microscopists, was well known and appreciated by the members of the Section.

Mr. LINTON exhibited some neatly executed drawings and mounted slides of a Coccus found on an orange sent to him by Mr. Heys. The late Mr. R. Beck discovered this Coccus to be of the same species as those found on a Cotoneaster and Rosebush in his garden.

The PRESIDENT exhibited two erecting eyepieces for the microscope, by Nachet of Paris. Instead of placing the erector between the eyepiece and object glass, as is usual in the English instruments, Nachet combines the erector and eyepiece together. In one form the prism is placed between the eye lens and the field lens of the eyepiece; in the second form the prism is placed over the eye lens of the eyepiece.

He also exhibited a very portable compound microscope for travellers' use.

Mr. LINTON exhibited some live Diatoms from Southport.

PHYSICAL AND MATHEMATICAL SECTION.

February 25th, 1868.

E. W. Binney, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

"On the Aurora Borealis as a Weather Prognostic," by Thomas Ogilvy, Esq. Communicated by Murray Gladstone, F.R.A.S.

It is upwards of 36 years since my attention was directed to the aurora borealis as a weather prognostic.

I was then residing in Cheshire, having frequently to cross the river Mersey, and ride or drive several miles after dusk, which gave much opportunity of observing the aurora, and of remarking the nature of the weather which accompanied and followed its appearance.

I think it was generally seen when the wind, if any, was light; and in Winter, inclined to be northerly, with a tendency to frost; and when the corruscations were vivid, and particularly if extending towards the zenith, or showing much motion, I remarked that the aurora was *almost invariably* followed by a gale of wind, with rain, from S.W.—within from forty-eight hours, to four days.

The more brilliant and lively the appearance and motion of the aurora, the earlier the gale which followed took place, and the greater was its severity. Slighter manifestations of the northern lights were not followed by any appreciable change of weather.

One of the most striking appearances of the aurora I ever witnessed was at Corrimony, on the 18th November, 1848. It was of a deep fiery red colour, and extended all over the heavens; even from the zenith down to the summits of the southern hills; and within two days it was succeeded by one of the most violent storms of wind and rain from the S.W., I ever witnessed in that part of the kingdom.

It was only within the last four or five years that I met with an extract from a newspaper; published, at the time, in one of the midland counties of England; in which reference was made to the same appearance of the aurora borealis; on the same date, and about the same hour in the evening, eight or nine o'clock. It gave a similar description as to colour, extent over the visible horizon, &c., but added that it was not followed, "as usual," by westerly wind and rain. I would call attention to this fact, as it serves to illustrate a theory I would offer in explanation of the phenomenon generally.

I would only further notice an observation somewhat different in kind, and made during the day; it was mentioned by my grieve, soon after I went to reside in Elen Urquhart, 26 years ago; and he was one of the most observant, and intelligent men of his class I ever met with.

On a brilliant afternoon towards the end of Summer, I happened to remark to him, how very fine the weather had been and was, when, shading his eyes from the Sun, and looking up over head, he replied that, notwithstanding, there would be rain in three days, "for he saw it in the sky;" adding, he found this almost always right. On inquiry, and looking up in a similar way, I discovered that what he saw was a faint streaky cloud, nearly in the zenith, with a shooting and sort of wavy motion towards its southern margin.

This prediction was verified in the instance here referred

to; and as often, afterwards, as I happened to remark the same appearances, but my attention was never so much, nor from its nature, so often directed to this as to the nocturnal phenomenon; and I do not, from my own experience, speak of it with the same confidence—but I cannot resist the conclusion that it is the same meteor, only presenting a different appearance as seen at night, and in bright sunlight.

From the frequency with which the aurora was at one time brought before me, I was gradually led to form some theory explanatory of its nature, and of the weather changes I had observed to succeed it, which I here submit, with diffidence, for consideration.

I would first notice a remark made, if I recollect aright, by Sir John Ross, the Arctic navigator, that the aurora was confined to the lower (or *lowest*) regions of the atmosphere.

That this should be the case in very high northern latitudes, would be quite in accordance with the theory I have formed. That the reverse is the case in our own country may be inferred from the appearances in Inverness-shire, and in England in November, 1848;—and, I believe, from many similar phenomena being observed, at the same times, over wide ranges of the lower latitudes. But this, if correct, is equally in accordance with the theory.

In explanation, I will suppose it may be assumed that the air passes from the poles towards the Equator; where, being rarified, it rises, and, in considerable degree, at least, returns back to the poles, in the higher regions of the atmosphere.

This general circulation is, no doubt, much modified nearer the surface of the globe, by large islands, chains of mountains, and more especially by extensive continents, as they are heated or cooled down by the comparative presence or absence of the sun's rays. But where not influenced by such cases, and in those latitudes where they have room to expand and find

uninterrupted course over a wide and unbroken extent of ocean, the unvarying direction and perennial nature of the N.E. and S.E. trade winds appears to prove the fact of a constant flow of air from the poles towards the equator. On their approaching the equator, these winds, as might be anticipated, enter a zone of frequent calms, and are lost. This constant supply of air, poured from both poles towards the equator, must find vent in some way; and while running down the N.E. trade winds on a voyage to India, I have repeatedly observed, through the intervals of the lower clouds, detached thin ones moving at a great elevation towards the north, and directly against the course of the trade winds and of the much lower but heavier stratum of vapour carried along with them.

At the changes of monsoons, the opposite winds are found charged, respectively, with negative and positive electricity, and heavy storms of thunder and lightning result on their meeting; and the same is observed in a minor degree when easterly and westerly winds meet. Magnetism, so powerful in the higher latitudes, is weakened towards the equator; and if it may be inferred that, in like manner, the positive electricity brought by the atmosphere from the poles is discharged on approaching the equator and becomes negative, an explanation would be afforded of the cause and nature of the aurora borealis.

This is distinctly given as his opinion by the late Admiral Fitzroy, in his report of 1864, on the Polar and Equatorial Air Currents.*

* "We have also found the telegraph wires much disturbed when these main (principal) currents were beginning to act positively in force. No great tempest—no heavy polar or tropical gale has occurred here since 1860 without more or less '*wire disturbance*' (as the telegraphists say); and I am led by these, with other concomitant facts, and examination of numerous sea records, as well as registers on land, to believe that such electric effects are caused by atmospheric collision, and that '*magnetic storms*,' sedulously watched now at many observatories, are *results* of atmospheric storms,

When a larger body than usual of light air from the south begins to descend upon the upper surface of the stream from the north, as those opposite currents in the atmosphere come into close proximity, their negative and positive electricities produce corruscations.

The rarity of the atmosphere and the great elevation probably prevent (at least for the most part) any sound or thunder being heard; and the former cause, joined with the manner in which the currents approach each other, may probably occasion the shooting, flickering movements of the aurora, and of the clouds formerly mentioned as being seen by day.

The arches of boreal light frequently seen stretching from E. to W., may be produced by large masses of air, charged with opposite electricities, meeting each other and feeding the flame, quietly and continuously, on an extended front; while the movements of light occasionally occurring throughout the length of these arches, may arise from the masses of vapour coming more actively into contact at particular points, and lighting up a corruscation, which like a running fire, passes along the whole line.

When the corruscations are more than usually vivid, or violent in their motion, it would indicate a larger arrival than usual of negatively electric air from S. or S.W., which, *in a shorter or longer time, according to its strength,* first checks, and then overpowers the N. or N.E. wind generally blowing when the aurora is seen. The lower temperature of the atmosphere, cooled down by the recent northerly wind, condenses the moisture borne by that from the warm

somewhere in one hemisphere or other, instantaneously felt through the air, however remote—or by conduction, or by vibration—as an earthquake is felt on board ship in the middle of a deep ocean, even the South Pacific."

The Rev. P. Secchi, Director of the Roman Observatory, in writing of magnetic currents in a wire in the magnetic meridian, states that disturbances in it indicate the approach of storms, as well as the barometer.

south, and precipitates it in showers; or, in more extreme cases, in storms of rain.

If I do not mistake, Sir John Ross describes the aurora in high northern latitudes, in winter, as being frequent, almost constant, as well as *very* near the Earth.

The cause of this seems to be the opposing currents being then close to the point where they must necessarily come into contact, and where even the smallest portion remaining of the opposite electricity must be discharged.

Sir John also speaks of the waving motion being accompanied by a very perceptibly audible sound. This may be aided by the known property of highly condensed air to transmit sound to unusual distances; and I have a strong impression of having myself heard a sound similar to that produced by the waving of a flag, when the aurora was playing actively.

As far as it is known, the identity of the light flickering clouds, above described as seen by day, and of the aurora as visible at night, is a *new* observation; and the following opinions, expressed by men of great eminence in meteorological science, while in no instance identical with the theory here proposed, may it appears, be not unfairly adduced in its support. Beccaria, in the last century, offered a suggestion that there was a constant circulation of the electric fluid between north and south; but he gave an explanation different from the foregoing of the manner in which he conceived it might take place. Herschel states his idea, in general terms, that the northern lights are produced by the electric fluid; and Admiral Fitzroy, in his report referred to above, "On the Polar and Equatorial Air Currents," has the following passage:—"We find here that the main currents, polar and equatorial, have very different electrical characteristics—one, the polar, being always plus; the other, always minus;—if pure, unmixed with the polar."

There is, besides, the well-known experiment of passing an electric spark, in vacuo, through a glass tube, when it presents the appearance of the aurora borealis, even with its play.

I would only further advert to the fact, mentioned above, that the remarkable aurora in November, 1848, was not followed by rain, where seen in England; while within two days after its occurrence, there was a perfect deluge of rain, and almost a hurricane, in Inverness-shire.

If, on reaching a few degrees further north, the column of air from the south had come into contact with that from the north in sufficient force to drive it back on the surface of the earth; in other words, had proceeded so much farther downwards on its course to its ultimate depression near the pole, as thereby (in consequence of its necessary contact with the colder northern atmosphere) to condense and discharge the vapour with which it was itself loaded, but which it was too high up to get rid of, when seen in the centre of England; if this were the case with that remarkable aurora borealis, may it not likewise, without unfairness be deemed a corroboration of the theory suggested?

"On the Supposed Influence of the Moon on the Temperature of the Atmosphere near the Surface of the Earth," by Joseph Baxendell, F.R.A.S.

The January number of the "Monthly Notices" of the Royal Astronomical Society contains a paper entitled "Inductive Proof of the Moon's Insolation," by Mr. J. Park Harrison, M.A., in which the Author points out that when the mean daily temperatures at Berlin for the years 1820—1835; at Oxford for 1856—64; and at Greenwich for 1841—47 and 1856—64, are arranged in tables according to the age of the moon, they show that a maximum mean temperature occurs on the average, at each of the stations, upon the 6th and 7th day

of the lunation, when the moon's crust turned towards the earth is coldest; and a minimum mean temperature some time after full moon, when the same crust has been exposed for several days to intense radiation from the sun. It does not appear that any other period than that of the moon's synodical revolution was tried; nor is any allusion made to the temperature period which, as Nervander and Buys Ballot showed many years ago, depends apparently upon the sun's geocentric rotation on his axis. The existence of both these periods has however been questioned. Thus Buys Ballot on tabulating a series of 70 years' mean daily temperatures according to the moon's age found that the highest temperatures occurred during the seven days after full moon, or precisely during that part of the lunar period in which, according to Mr. Harrison's results, the temperature is at a minimum; and the Astronomer Royal discussing the observations of temperature made at Greenwich from the 2nd of June, 1848, to the end of 1853, with reference to the period of the sun's geocentric rotation on his axis, obtained a series of numbers "which did not appear to support the hypothesis that there is any certain inequality of temperature depending on the meridian of the sun which is turned towards the earth." In an inquiry of this nature it is, however, manifestly very undesirable to rely upon observations taken during the same series of years at places so near each other as Greenwich and Oxford, or upon those taken at places far apart, but in different years. The general agreement between the results of the Berlin observations in 1820 to 1835, and those at Oxford in 1856 to 1864, as given by Mr. Harrison is, of itself, no proof that the temperature of the earth's atmosphere is sensibly affected by the moon. This agreement may, and indeed does, arise from causes altogether independent of any influence of the moon. In a paper which I communicated to this Society in March, 1864, "On Periodic Changes in the

Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface."* I showed that different years have temperature periods of different lengths ranging from about 23½ to 31 days, the minimum values occurring in years of minimum solar spot frequency, and the maximum in years when the solar spots are most numerous; the approximate mean value as derived from observations made at various stations in Europe, Asia, and North America, during the years 1833 to 1859, being 27·4 days; and it was also shown that although in any particular year observations at places scattered widely over the three Continents, gave the same value of the temperature period yet the epochs of maximum and minimum often differed very considerably, thus indicating that the variation was not due to any direct action of the sun, the effects of which would have occurred simultaneously at all the stations. The adoption of any fixed value of a temperature period whether that of the sun's geocentric rotation on his axis, or of the moon's synodical revolution round the earth, or any assumed period, did not satisfactorily represent observations made in different years, and gave much greater probable errors than a variable temperature period whose changing values followed the course of the curve of sun spot frequency; and thus while the results given by the Astronomer Royal in the "Astronomische Nachrichten," No. 934, do not support the hypothesis of a temperature period depending on the sun's rotation on his axis, they will, nevertheless, on careful examination be found to indicate changes of temperature occurring in a period which was gradually diminishing from the com-

* Proceedings, Vol. III., p. 251—260.

mencement of the series of observations in 1848 to its termination in 1853. On the other hand Mr. Harrison's results appear to support very strongly the hypothesis of a lunar period; but it is easy to show that they form no exception to the same general law. For this purpose I have made use of the St. Petersburg observations for 1856—64. being the years comprised in Mr. Harrison's Oxford series; and as the diagrams which accompany Mr. Harrison's paper show a maximum of mean daily temperature on the 7th day after new moon, and a minimum on the 5th day after full moon, I have arranged the St. Petersburg mean daily temperatures for the five days from the 5th to the 9th day after new moon, and the five days from the 3rd to the 7th after full moon in two tables. There were 111 lunations during the nine years, and the mean temperatures of the selected days were found to be as follows :—

Days after New Moon.	Mean Tem.	Days after Full Moon.	Mean Tem.
5th day.....	38·32° F.	3rd day	38·88° F.
6th „	38·30	4th „	38·79
7th „	38·57	5th „	39·29
8th „	38·16	6th „	39·62
9th „	37·65	7th „	39·96
Mean.....	38·20	Mean.....	39·31

The temperatures of the days in the group after new moon, are all below the lowest in the group after full moon; and the mean of the first group is 1·11° below that of the second; but at Oxford, during the same series of years, the highest temperatures occurred on the days comprised in the first group, and the lowest on those in the second. It is evident, therefore, that Mr. Harrison's results cannot be

regarded as a proof of the moon's insolation; or that it has any sensible effect upon the temperature of the atmosphere near the surface of the earth.

I may here remark that the results given by Mr. Glaisher, F.R.S., in his paper "On the Influence of the Moon on the Direction of the Wind" (Proc. Met. Soc., March, 1867), are also dependent upon the general law above referred to, and cannot fairly be regarded as proving that the moon has really any influence upon the direction of the wind, since in most years, other periods than that of the moon's synodical revolution give greater differences in the distribution of the winds than can be obtained by employing the lunar period.

"On the Rainfall at Old Trafford, Manchester, during the year 1867," by G. V. Vernon, F.R.A.S., F.M.S.

The rainfall for 1867 was 0·858 inches below the average of the last 74 years, and 8·590 below the fall for 1866. The fall occurred on 188 days in 1867, as against 214 days in 1866.

During the first three months of the year the fall somewhat exceeded the average. In the second three months the fall was above the average, but this was caused by a very wet April, the fall in this month being more than double the average, whilst in the other two months the fall was below the average, especially in June. In the three months ending September, the fall was considerably below the average. July was wet, but August and September were below the average, August not having one-half the usual fall.

In the last three months of the year, the fall was slightly below the average; October exceeded the average slightly,

November was considerably below the average, and not half the amount that fell in November, 1866, whilst December exceeded the average. The daily falls exceeding half an inch were :—

1867.	Inches.
January 7	0·500
April 20	0·630
July 15	0·770
July 16	1·110
September 1	0·620
October 7	0·520
October 27	0·750
November 14	0·510
November 30	1·500
December 1	0·500
December 15	0·800
December 20	0·540

In 1867 we had only two days upon which the daily amount exceeded an inch, whilst in 1866 the daily fall exceeded an inch upon five days, and upon one day, viz. :— 28th July, 1866, it was 1·985 in.

It would seem that in our district we can safely reckon upon having rain on an average of about 180 days in the year, or that rain falls on about half the days of the year. Of course this includes every day upon which a measurable amount of rain falls, and it is only right that every day on which rain falls, and can be measured, ought to be included, notwithstanding Mr. Symons' limiting the amount to days only on which the fall reaches 0·010 inch. In 1867, there were no less than 16 days on which rain fell less than 0·010 inch in amount, and to leave these out must vitiate the result. Moreover, in places where the rainfall is much less

than here, there will probably be a much greater number of days upon which a daily amount of 0·010 inch falls. Every day upon which a measurable amount of rain falls ought to be given; and if for any other purpose a limit in amount is wanted, it ought to be tabulated separately.

Old Trafford, Manchester.

Rain Gauge 3 feet above the ground, and 106 feet above mean sea level.

Quarterly Periods.		1867	Fall in Inches.	Average of 74 Yrs.	Differences.	No. of Days' Rainfall in 1867.	Quarterly Periods.		Quarterly Periods for 74 Yrs.
1866	1867						1867	1866	
			In.	In.	In.		In.	In.	In.
		Jan.....	3·270	2·490	+0·780	15			
59	42	Feb.....	2·930	2·394	+0·536	12	7·646	8·403	7·171
		March...	1·446	2·237	—0·841	15			
		April ...	4·323	2·032	+2·291	25			
32	47	May ...	1·950	2·351	—0·401	9	7·864	5·814	7·266
		June ...	1·591	2·883	—1·292	13			
		July ...	4·284	3·625	+0·659	15			
67	50	August..	1·410	3·586	—2·176	16	8·684	16·556	10·461
		Sept. ...	2·990	3·250	—0·260	19			
		Oct.....	3·979	3·811	+0·168	22			
56	49	Nov. ...	2·431	3·480	—1·049	8	10·385	12·396	10·539
		Dec.....	3·975	3·248	+0·727	19			
214	188		34·579	35·437	—0·858	188	34·579	43·169	35·437

Ordinary Meeting, March 17th, 1868.

Edw. Schunck, Ph.D., F.R.S., &c., President, in the Chair.

E. W. Binney, F.R.S., F.G.S., exhibited to the members an original letter of the late Sir Walter Scott, written many years ago to a gentleman residing in the North Riding of Yorkshire, on the subject of an old ballad entitled "*Jock o' Milk*":—

Sir,

You have doubtless by this time set me down as guilty of great ingratitude and unworthy of your farther correspondence for so long and unjustifiable a delay in answering your letter enclosing *Jock o' Milk*. The truth is I have been absent from Edinburgh for some weeks, and since my return my professional engagements have obliged me to leave the tales of the East, West, and Middle Marches as quiet in my desk as the bodies of their quondam heroes rest in their graves. At length I have an opportunity to acknowledge your obliging favour. My Incredulity with regard to the Ballad you have been so good as to send me is not yet entirely obviated. If it is not entirely and radically a modern fabrication, the ancient verses are what the ffrench call *beaucoup brodeés*. "Virtue is its own reward," trite as the sentiment is, can hardly be supposed quite so old as the reign of Dav^{d} II. The Title of Duke was first introduced into Scotland in the Reign of Rob^{t} 3^{d}, and was only conferred upon immediate relations of the Royal family till at a very late period the Hamilton family got that title.

There never was, as far as I can learn, a peer, whether Duke, Earl, or Baron, of the name of Irving, and although there were many landholders of the name in the south-west of Scotland, the principal seat of their Chieftain was *Drum*, in Aberdeenshire. So far with regard to historical fact—but a ballad maker is entitled to use great latitudes in that respect, and accordingly it is not upon the anachronisms that I chiefly found my disbelief in the antiquity of the Poem. It is rather upon the mixture of ancient and modern phraseology, and especially upon the different attempts at sentiment and pathos, inconsistent with the simplicity of the Minstrel style, that I ground my opinion, which will always, however, be subject to alteration upon reasonable and convincing evidence. The copy you have been so good as to send me is nearly the same with one which I found in Glenriddell's MS. collection of ballads, and with another procured from Mr. David Herd, of this place. The last copy has this memorandum—"This fragment was taken down from the recitation of some of the country people in Annandale, by William Bell, a writer there, who communicated it to D. H., but in a very bad case, about the year 1776, and he was afterwards informed that Dr. Clapperton, a surgeon, in Lochmaben, was in possession of a compleat copy of the Ballad, which never could be got, the Dr. intending, as was said, its publication along with several other curious ancient songs." As this account in a great measure tallies with that with which you have favoured me, I hope it may be yet possible to recover some account of the original copy of this curious Ballad, by which means we may perhaps be able to determine what parts are modern and what really ancient. I shall wait with impatience the result of your inquiries at your friend Mr. Lidderdale. The battle in question, if such there was, must have been fought in the course of the four years intervening betwixt 1342, the date of David's return from France, and 1346, when the fatal battle of Durham was

fought, in which Randolph E. of Murray was slain, and the King himself led into captivity.—Believe me, Sir, with many thanks for your obliging communication, your faithful humble servt.,

Edin., 4 June, 1802. WALTER SCOTT.

"Note on 'An Essay on the Resolution of Algebraic Equations, by the late Judge Hargreave, LL.D., F.R.S.,'" by the Rev. THOS. P. KIRKMAN, M.A., F.R.S.

The following appears to me to be a demonstration, that Dr. Hargreave's very skilful attempt to solve the quintic is, like all that have preceded it, a failure:

One of the final equations at p. 94 is—

$$y_1^{\frac{1}{5}}+y_2^{\frac{1}{5}}=A_1(z_1-b_1)+A_2(z_2-b_1)+A_3(z_3-b_1)+A_4(z_4-b_1)+A_5(z_5-b_1)$$
$$=\Sigma(At).$$

If so, it is possible to find $a_1+\alpha_1=A_1$, $a_2+\alpha_2=A_2$, &c., such that

$$y_1^{\frac{1}{5}}+y_2^{\frac{1}{5}}=\left((\Sigma at)^5\right)^{\frac{1}{5}}+\left((\Sigma\alpha t)^{\frac{1}{5}}\right)=\left(P^5\right)^{\frac{1}{5}}+\left(Q^5\right)^{\frac{1}{5}},\text{ where}$$
$$P^5=y_1\text{ and }Q^5=y_2.$$

As $y_1+y_2=2(51)$, p. 85, is a rational and symmetrical function of the z's, P^5+Q^5 is one also; and this is plainly impossible, unless $P+Q=\Sigma At$ is a symmetrical function of the z's, That is

$$y_1^{\frac{1}{5}}+y_2^{\frac{1}{5}}=\Sigma(At)=A\Sigma t=A\Sigma(z-b_1)=0;\text{ and } y_1=-y_2;$$
$$\text{or } y_1+y_2=2(51)=0,$$

This destroys the form of the conditioned quintic in z, at p 82; for $(51)=0$ gives, as (21) and (31) are each $=0$, (p. 78), (p. 7), $(51)=4b_1^5-5b_1b_4+b_5=0$; or b_5 is expressible in b_1 and b_4.

I suspect that it follows in a similar way from the equation in p. 54, that $(41)=0$, in the quartic in z in p. 53.

It is evident from consideration of the known solutions of the quartic, the cubic, and the quadratic, that every solution of the general quintic $F=0$, whose roots are $x_0\ x_1\ x_2\ x_3\ x_4$, must be a transformation, by algebraic artifice, of the expression

$$5x_0=\Sigma x+4x_0+(\alpha+\alpha^2+\alpha^3+\alpha^4)(x_1+x_2+x_3+x_4);$$

where α is an imaginary fifth root of unity; and any transformation, by which the right side of this expression can be made to contain only symmetrical functions of $x_0\ x_1\ x_2\ x_3\ x_4$, will be, by proper reading of roots, a correct solution of F=0, as x_0 is any one of the five. Let us try to effect this. We can write

$$x_0=\frac{1}{5}\Sigma x+\frac{1}{5}\left((x_0+\alpha_1x_1+\alpha_2x_2+\alpha_3x_3+\alpha_4x_4)^5\right)^{\frac{1}{5}}$$
$$+\frac{1}{5}\left((x_0+\alpha_1x_2+\alpha_2x_3+\alpha_3x_4+\alpha_4x_1)^5\right)^{\frac{1}{5}}$$
$$+\frac{1}{5}\left((x_0+\alpha_1x_3+\alpha_2x_4+\alpha_3x_1+\alpha_4x_2)^5\right)^{\frac{1}{5}}$$
$$+\frac{1}{5}\left((x_0+\alpha_1x_4+\alpha_2x_1+\alpha_3x_2+\alpha_4x_3)^5\right)^{\frac{1}{5}},$$
$$=\frac{1}{5}\Sigma x+\frac{1}{5}\left(\mathrm{P}_1^5\right)^{\frac{1}{5}}+\frac{1}{5}\left(\mathrm{P}_2^5\right)^{\frac{1}{5}}+\frac{1}{5}\left(\mathrm{P}_3^5\right)^{\frac{1}{5}}+\frac{1}{5}\left(\mathrm{P}_4^5\right)^{\frac{1}{5}}$$
$$=\frac{1}{5}\Sigma x+\frac{1}{5}\left(\mathrm{S}+\mathrm{H}_1\right)^{\frac{1}{5}}+\frac{1}{5}\left(\mathrm{S}+\mathrm{H}_2\right)^{\frac{1}{5}}$$
$$+\frac{1}{5}\left(\mathrm{S}+\mathrm{H}_3\right)^{\frac{1}{5}}+\frac{1}{5}\left(\mathrm{S}+\mathrm{H}_4\right)^{\frac{1}{5}};$$

where S is a symmetric function of the roots of F=0, and $\mathrm{H}_1\ \mathrm{H}_2\ \mathrm{H}_3\ \mathrm{H}_4$ are four values of a 24-valued function of these roots. H_2 is formed from H_1 by the substitution $\begin{smallmatrix}0&1&2&3&4\\0&1&2&3&4\end{smallmatrix}1$, effected on the subindices of the x's, leaving undisturbed the $\alpha\ \alpha^2$, &c.; H_3 from H_1 by the substitution $\begin{smallmatrix}0&1&2&3&4\\0&1&2&3&4\end{smallmatrix}2$, and H_4 from H_1 by $\begin{smallmatrix}0&1&2&3&4\\0&1&2&3&4\end{smallmatrix}3$. H_1 is unaltered by the cyclical permutation of $x_0\ x_1\ x_2\ x_3\ x_4$.

Another set of four values, $\mathrm{H}_5\ \mathrm{H}_6\ \mathrm{H}_7\ \mathrm{H}_8$, can be formed by operating on $\mathrm{H}_1\ \mathrm{H}_2\ \mathrm{H}_3\ \mathrm{H}_4$ by the substitution $\begin{smallmatrix}0&1&2&3&4\\0&1&3&4&2\end{smallmatrix}$, or, which is the same thing, by operating on H_1 only, by the four substitutions 01342, 03421, 04213, 02134, omitting the suffixed 01234. And if we write in the above expression for x_0, $\mathrm{H}_5\ \mathrm{H}_6\ \mathrm{H}_7\ \mathrm{H}_8$ in the places of $\mathrm{H}_1\ \mathrm{H}_2\ \mathrm{H}_3\ \mathrm{H}_4$, we shall have still a correct expression of the value of x_0. Is not this a sufficient and a new demonstration of the absurdity of supposing a solution of the quintic?

Four more sets each of four values of H_1 can be made by operating on $\mathrm{H}_1\,\mathrm{H}_2\,\mathrm{H}_3\,\mathrm{H}_4$, by the four substitutions, 01423, 01243, 01432, 01324; and the sum of the 24 values of H_1, thus obtained in sets

of four, will be that of all the values that H_1 can have by permutation of the x's, leaving untouched the $a\,a^2$, &c. We thus obtain six distinct expressions for a_0, of which no two contain a common value of the 24-valued H_1. All this is true if $x_0=\phi t_0$, $x_1=\phi t_1$, $x_2=\phi t_2$, &c., H_1 being a function of the ϕt's, and S also.

It is plain that we can form the equation

$$y^{24}+ay^{23}+by^{22}+\text{ \&c.}\qquad =0=U,$$

whose roots are these 24 values of H_1, and that a, b, &c., in this equation will be rational functions of the coefficients of $F=0$. If we knew how to solve this equation, $U=0$, we should have the correct solution of the quintic $F=0$, by writing, instead of $H_1\,H_2\,H_3\,H_4$, the algebraic root of $U=0$, under each of the four quintic radical signs. The ambiguity arising from the different roots of unity read with all the radicals would of course remain, as in the lower equations.

I see but two conceivable chances of success, either that $U=0$ should break up readily into six biquadratics, or that it should be somehow reducible to the form

$$y^{24}+Ky^{18}+Ly^{12}+My^6+N=0=U^1,$$

or to some other biquadratic in z, z being a cubic in y^2.

Such reduction would be possible if we had a sufficient number of constants at our disposal in $x_0\,x_1\,x_2\,x_3\,x_4$. Let us then suppose that, instead of the general quintic $F=0$, we have a quintic $F^1=0$ to solve, whose roots are ϕx_0, ϕx_1, ϕx_2, ϕx_3, ϕx_4, $x_0\,x_1$ &c. being the roots of $F=0$, and

$$\phi x=hx^4+kx^3+lx^2+mx+n,$$

$h\ k\ l\ m\ n$ being undetermined quantities, surd or rational. We can write

$$\phi x_0=\frac{1}{5}\Sigma\phi x+\frac{1}{5}\left(S+H_1\right)^{\frac{1}{5}}+\frac{1}{5}\left(S+H_2\right)^{\frac{1}{5}}+\frac{1}{5}\left(S+H_3\right)^{\frac{1}{5}}+\frac{1}{5}\left(S+H_4\right)^{\frac{1}{5}}$$

where S is a symmetrical, and H_1 H_2 H_3 H_4 are values of a 24-valued, function of the ϕx's. We can form, as before, the equation $U=0$, whose roots are the 24 values of this H_1 and in all whose coefficients the constants $h\ k\ l\ m\ n$ are at our disposal. If by solutions of equations among these quantities, of degrees below

the fifth, we can reduce U to the form U^1, we have ϕx_0 by putting for $H_1\ H_2\ H_2\ H_4$ the given algebraic expressions

$$Y_1^{\frac{1}{6}},\quad Y_2^{\frac{1}{6}},\quad Y_3^{\frac{1}{6}},\quad Y_4^{\frac{1}{6}};$$

Y_1, &c., being the four roots of $U^1=0$. The roots of $F=0$ would be then given by five biquadratics, $h\ k\ l\ m\ n$ being now determined functions of the coefficients of $F=0$ through those of $F^1=0$,

$$\phi x_0=C_0,\ \phi x_1=C_1,\ \phi x_2=C_2,\ \phi x_3=C_3,\ \phi x_4=C_4;$$

and Dr. Hargreave's device would bear the coveted fruit.

Without expressing any opinion about the possibility of this transformation, I can shew that the equation $U=0$ is farther conditioned beyond what has been seen concerning the arrangement of its roots in sets of four. We formed at first $H_1\ H_2\ H_3\ H_4$ on the first of the following groups :—

1234	1234	1234	1234
2341	3142	4312	3412
3412	4321	2143	4321
4123	2413	3421	2143

If we had used the second instead of the first group, we should have formed $H_1\ H_2^{\rm I}\ H_3^{\rm I}\ H_4^{\rm I}$, of which H_1 only is in the former set of four: if we had used the third, we should have formed $H_1\ H_2^{\rm II}\ H_3^{\rm II}\ H_4^{\rm II}$, all fresh values, except H_1: if we had employed the fourth, we should have formed $H_1\ H_3\ H_3^{\rm I}\ H_3^{\rm II}$, all before written. Thus we see that $U=0$ is something very different from the general equation of its degree. It may possibly break up into quadratics, by introducing surds into the coefficients.

Whichever group we employ, its five derivates give us five more expressions of x_0; that is, there are 24 ways of expressing x_0, exhibiting 24 different quadruplets from the 24 values of H_1.

I fear much that all this is rather a proof of the absurdity of attempting, than a contribution towards effecting, the algebraic resolution of the quintic. But it may be worth the while of some one, endued with more faith, to form and to study this $U=0$ of the 24th degree. He would deserve well of science who could shew us how to form it by a compendious method.

When there are only four roots, $x_0\ x_1\ x_2\ x_3$, we obtain, if we

choose needlessly to introduce three biquadratic instead of the usual three quadratic radicals free from $\sqrt{-1}$, after Σx,

$$4x_0=\Sigma x+\left(S+H_1\right)^{\frac{1}{4}}+\left(S+H_2\right)^{\frac{1}{4}}+\left(S+H_3\right)^{\frac{1}{4}},$$

where $S=\Sigma x^4+24\cdot x_0x_1x_2x_3-6\Sigma x^2x^2$, and

$$H_1=12(x_0^2x_2^2+x_1^2x_3^2)-4\Big(x_0x_2(x_0^2+x_2^2)+x_1x_3(x_1^2+x_3^2)\Big)$$
$$+4\sqrt{-1}\Big(x_0^3(x_1-x_3)+x_1^3(x_2-x_0)+x_2^3(x_3-x_1)+x_3^3(x_0-x_2)\Big),$$
$$=h+4k,$$

h being a three-valued, and k a six-valued function, whose square k^2 is three-valued. We can form with the coefficients of $F=0$ the two cubics whose roots are the values of h and the values of k^2. If their algebraic roots be $Y_1\ Y_2\ Y_3$, and $Z_1\ Z_2\ Z_3$, we have, for the root of the biquadratic

$$F=(x-x_0)(x-x_1)(x-x_2)(x-x_3)=0,$$

$$x_0=\frac{1}{4}\Sigma x+\frac{1}{4}\Big(S+Y_1+4Z_1^{\frac{1}{2}}\Big)^{\frac{1}{4}}+\frac{1}{4}\Big(S+Y_2+4Z_2^{\frac{1}{2}}\Big)^{\frac{1}{4}}+\frac{1}{4}\Big(S+Y_3+4Z_3^{\frac{1}{2}}\Big)^{\frac{1}{4}}.$$

To transform into this shape the ordinary solution does not seem easy.

It is obvious that x_0 can be represented in like manner in terms of $x_0\ x_1\ x_2\cdots x_{n-1}$ by an expression which is true when for x_0 is put any one of the $n-1$ others. Nothing more is required for the algebraic solution of the general equation of the nth degree, than to break up the given equation $U=0$ of the degree $(n-1)\cdot(n-2)\cdots 2\cdot 1$ into factors of the $n-1$th degree. But the inexorable theory of groups forbids me to hope for this reduction of $U=0$, when n, the number of roots, exceeds four.

Has it been before pointed out, that the problem of algebraic resolution of equations is merely that of elimination of imaginaries by involution into symmetrical functions under radical signs?

"On the Origin of Colour and the Theory of Light," Part II., by J. Smith, M.A., Perth Academy.

On a former occasion the Author proved experimentally

that colour can be produced by the mere motion of a white ray without the aid of refraction; his object now was to explain those physical laws by which colour is produced from a homogeneous substance.

He argued that as we hear through the medium of the air—a homogeneous substance—and the variety of sounds is equal to the variety of colours, we may have as great a variety of colour from the ether considered as homogeneous as of sounds from the air so considered.

The difficulty was to prove by what process nature modifies so wonderfully the motion of a homogeneous substance. Many years ago he found that by pressing a piece of glass with a point he obtained colours. If the glass pressed were laid on an irregular surface, as a piece of ivory or steel, the colours were in irregular patches; but if the glass were laid on another piece of glass, the circles were complete.

This proved that Newton's rings, as they are called, were produced by undulations or waves in the glass, that these waves are virtually small prisms, and that they are stationary as long as the pressure is stationary.

By and by he found that similar waves or bands exhibited themselves in a film of soap when made to cover the mouth of a tumbler, and that they were particularly interesting and instructive when the tumbler was held at an angle to the horizon. He also found that by blowing through a tube on a plate of glass, a converse series of rings was obtained. And as a proof that these rings were formed of waves, we had merely to examine their motions—the motion of the one series was the converse of the other.

Again, he took a thin disc of glass and laid it on another piece of glass of any thickness, and subjecting it to pressure

and then cementing the pieces together at the edges, the rings were made permanent.

He also showed how the parallel bands of colour could be imitated by prismatic refraction of white bands of the same ratio as to breadth.

The author then applied these laws to explaining the phenomena of the prism.

He argued that all matter was put in motion by light, just as we are sensible that every atom of a body is moved by heat. He showed, not only by diagrams but mechanically, the physical laws by which light was refracted and reflected, and expounded, as he considered, the true theory of prismatic refraction.

As — how a ray of light was always refracted towards the base of a prism, and how it could not be physically otherwise. Then from these laws he concluded — Given the molecular vibration of matter, given the dispersion of light.

He consequently arrived at the conclusion that light no more passed through glass than does sound. If we could withdraw the ether from a glass globe, we could no more see through the glass than we could hear through it if the air were removed.

As to the ether being heterogeneous, we do not say the atmosphere is heterogeneous because one sound differs from another.

Why should light, then, differ from sound? Why compound, when the same uncompounded air can convey as great a variety of notes as there are shades or varieties of colours? A prism no more analyses light or the ether in

motion, than an Æolian harp analyses sound, or the air in motion, for light and sound are both merely modes of motion of two different elements.

As a magnet conveys motions of one kind, so glass conveys motions of another, the one not more wonderful than the other.

We surely consider the molecules of the magnet to vibrate. Why not the molecules of glass, and consequently of all matter refracting light ?

A telescope is but another kind of telegraph, only much more comprehensive in its range.

Ordinary Meeting, March 31st, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., President, in the Chair.

"On the Solution of Algebraic Equations," by the Rev. THOS. P. KIRKMAN, M.A., F.R.S.

I showed in the preceding number of these Proceedings that if $x_0\ x_1\ x_2\ x_3\ x_4$ are the roots of the quintic $F=0$, we can write,

$$x_0=\frac{1}{5}\Sigma x+\frac{1}{5}\left(S+H_1\right)^{\frac{1}{5}}+\frac{1}{5}\left(S+H_2\right)^{\frac{1}{5}}+\frac{1}{5}\left(S+H_3\right)^{\frac{1}{5}}+\frac{1}{5}\left(S+H_4\right)^{\frac{1}{5}},$$

where S is a symmetrical function of the roots, and $H_1\ H_2\ H_3\ H_4$ are four values of a 24-valued function of those roots. I can now show how to form these four values with the coefficients of $F=0$; *i.e.* how to solve the famous quintic by a method which leads directly to the solution of all algebraic equations. We write the complete group of 24 substitutions among four elements thus:

1234	1342	1423	1243	1324	1432
2143	3124	4123	2134	3142	4123
3412	4213	2314	4312	2413	3214
4321	2431	3241	3421	4231	2341
A	B	C	D	E	F

The 12 substitutions over A B C are all what we call *positive*, that is, they are made by an even number of transpositions; the 12 over D E F are all *negative*, that is, made by an odd number of transpositions. Thus 1243 is made by the transposition of 4 and 3; 2341 by the three transpositions 21, 31, 41. The systems (A+B+C) and (D+E+F) are changed one into the other by any odd number, and remain both unchanged by any even number, of transpositions.

The entire number of substitutions possible with r elements can be written down in like manner in a sum of two systems,

which both remain invariable by any even number, and are changed the one into the other by any odd number, of transpositions.

H_1 in the above expression for x_0 is invariable by the cyclical permutation of $x_0\ x_1\ x_2\ x_3\ x_4$, and therefore receives all its possible values by permutations of $x_1\ x_2\ x_3\ x_4$. H_2 is formed by effecting on H_1 the substitution 2143, H_3 by 3412, and H_4 by 4321, performed on the subindices of $x_1\ x_2\ x_3\ x_4$ in H_1.

Our object is to construct the values of $H_1\ H_2\ H_3\ H_4$ in terms of the coefficients of F=0.

The functions $H_1^i\ H_2^i\ H_3^i\ H_4^i$ can all be formed for any integer index i. Let

$$\begin{aligned} H_1^i + H_2^i + H_3^i + H_4^i &= (A)_i \\ H_5^i + H_6^i + H_7^i + H_8^i &= (B)_i \\ \cdot \qquad\qquad & \cdot \\ \cdot \qquad\qquad & \cdot \\ \cdot \qquad\qquad & \cdot \\ H_{21}^i + H_{22}^i + H_{23}^i + H_{24}^i &= (F)_i \end{aligned}$$

where the four functions $(B)_i$ are formed by effecting on H_1^i the four substitutions written over B, &c.; or, which is the same thing, by raising to the ith power $H_5\ H_6\ H_7$ and H_8, made from H_1 by the four substitutions over B, &c.

It is plain that

$$(A)_i+(B)_i+(C)_i+(D)_i+(E)_i+(F)_i \quad =M_i$$

and

$$\Big((A)_i+(B)_i+(C)_i\Big)\Big((D)_i+(E)_i+(F)_i\Big)=N_i$$

are both given symmetrical functions of the roots, and rational in the coefficients of the quintic F=0; for the former is the sum of all the 24 values of H_1^i, and the latter is the product of two factors, either of which becomes the other by any odd number of transpositions of $x_1\ x_2\ x_3\ x_4$, and neither of which is changed by any even number of such transpositions.

Hence $(A)_i+(B)_i+(C)_i$ is the root of a given quadratic, whose coefficients are rational functions of those of the given quintic.

Next, the functions

$$\left\{(A)_i(B)_i+(A)_i(C)_i+(B)_i(C)_i\right\}+\left\{(D)_i(E)_i+(D)_i(F)_i+(E)_i(F)_i\right\}=P_i,$$

$$\left\{(A)_i(B)_i+(A)_i(C)_i+(B)_i(C)_i\right\}\left\{(D)_i(E)_i+(D)_i(F)_i+(E)_i(F)_i\right\} \quad =Q_i,$$

are rational functions of the coefficients of the quintic $F=0$; for the former is the sum, and the latter is the product, of two functions of $x_1 x_2 x_3 x_4$, of which either is changed into the other by any odd number of transpositions of those variables, while neither is changed by any even number of such transpositions.

Wherefore

$$(A)_i(B)_i+(A)_i(C)_i+(B)_i(C)_i$$

is the root of a given quadratic, whose coefficients are rational functions of those of the given quintic.

Finally,

$$(A)_i(B)_i(C)_i+(D)_i(E)_i(F)_i \quad =R_i \text{ and}$$

$$\left\{(A)_i(B)_i(C_i)\right\}\left\{(D)_i(E)_i(F)_i\right\}=S_i$$

are both rational functions of the coefficients of the quintic $F=0$, for the reasons above given, and $(A)_i(B)_i(C)_i$ is again the root of a given quadratic.

Hence we can form the cubic whose roots are $(A)_i$, $(B)_i$, and $(C)_i$, whatever be i; for its coefficients are given irrational functions of the coefficients of $F=0$ with ambiguous radicals.

Forming, then, and solving these cubics, for $i=1$, $i=2$, $i=3$, $i=4$, we obtain $(A)_i=V_i$, V_i being a given irrational function of the coefficients with ambiguous radicals. That is, we have

$$H_1+H_2+H_3+H_4=V_1,$$
$$H_1^2+H_2^2+H_3^2+H_4^2=V_2,$$
$$H_1^3+H_2^3+H_3^3+H_4^3=V_3,$$
$$H_1^4+H_2^4+H_3^4+H_4^4=V_4,$$

By the aid of these equations we can form the quartic whose roots are H_1, H_2, H_3, and H_4, by a well-known method; and, as we can also solve this quartic, we can write its four roots (H_1), (H_2), (H_3), (H_4), all given irrational functions of the co-efficients of $F=0$, in the above expression for x_0, in the places of H_1, H_2, H_3, and H_4; and thus we have the long-sought algebraic root of the given quintic $F=0$.

We cannot always form a positive and transitive group of $n-1$, corresponding to our group over A ; but the preceding reasoning applies with little change to the solution of the general equation of the nth degree. When n is a power of a prime number, the process will be somewhat abridged. Nothing prevents us from breaking up the function H_1 above handled into a number of separate functions, which will each contribute a quartic root to the solution, and thus most probably simplify the computation.

To solve the general sextic we proceed thus :—Let $x_0\, x_1\, x_2\, x_3\, x_4\, x_5$ be the roots of the sextic, G=0. We can write

$$x_0=\frac{1}{6}\Sigma x+\frac{1}{6}\Big(\{x_0-x_3+\alpha(x_1-x_4)+\alpha^2(x_2-x_5\}^6\Big)^{\frac{1}{6}}$$
$$+\frac{1}{6}\Big(\{x_0-x_4+\alpha(x_2-x_5)+\alpha^2(x_3-x_1\}^6\Big)^{\frac{1}{6}}$$
$$+\frac{1}{6}\Big(\{x_0-x_5+\alpha(x_3-x_1)+\alpha^2(x_4-x_2)\}^6\Big)^{\frac{1}{6}}$$
$$+\frac{1}{6}\Big(\{x_0-x_1+\alpha(x_5-x_2)+\alpha^2(x_5-x_3)\}^6\Big)^{\frac{1}{6}}$$
$$+\frac{1}{6}\Big(\{x_6-x_2+\alpha(x_6-x_3(+\alpha_2(x_1-x_4)\}^6\Big)^{\frac{1}{6}}$$
$$=\frac{1}{6}\Sigma x+\frac{1}{6}\Big(S+H_1\Big)^{\frac{1}{6}}+\frac{1}{6}\Big(S+H_2\Big)^{\frac{1}{6}}+\frac{1}{6}\Big(S+H_3\Big)^{\frac{1}{6}}$$
$$+\frac{1}{6}\Big(S+H_4\Big)^{\frac{1}{6}}+\Big(S+H_5\Big)^{\frac{1}{6}},$$

where α is an imaginary cube root of unity, and S is symmetrical in the roots and rational in the coefficients of G=0, and where $H_1\ H_2\ H_3\ H_4\ H_5$ are five values of a 120-valued function of those roots. Our object is to transform H_1, &c., into irrational functions of the coefficients of G=0.

Since $H_1\ H_2$, &c., *after* involution, are invariable by the cyclical permutations of $x_0\, x_1\, x_2\, x_3\, x_4\, x_5$, they receive all their possible values by the permutations of $x_1\ x_2\ x_3\ x_4\ x_5$.

The positive substitutions composed of five elements are as follows:—

j_1	k_1	l_1	m_1	
12345	21435	34125	43215	
23451	14352	41253	32154	
34512	43521	12534	21543	x_1,
45123	35214	25341	15432	
51234	52143	53412	54321	

j_2	k_2	l_2	m_2	
23145	32415	14235	41325	
31452	24153	42351	13254	
14523	41532	23514	32541	x_2,
45231	15324	35142	25413	
52314	53241	51423	54132	

j_3	k_3	l_3	m_3	
31245	13425	24315	42135	
12453	34251	43152	21354	
24531	42513	31524	13542	x_3.
45312	25134	15243	35421	
53124	51342	52431	54213	

The 60 negative substitutions are obtained by making the transposition 21 for 12 in all the preceding.

By
$$x_1 = j_1 + k_1 + l_1 + m_1$$
$$x_2 = j_2 + k_2 + l_2 + m_2$$
$$x_3 = j_3 + k_3 + l_3 + m_3$$

we mean to say that x_1 is the sum of the four subgroups written under j_1 k_1 l_1 and m_1, with a like signification for x_2 and x_3. And by $-x_1 = -j_1 + (-k_1) + (-l_1) + (-m_1)$, &c.,

we mean that $-x_1$, $-x_2$. $-x_3$, and $-j_1$, &c., are what x_1, x_2, x_3, and j_1, &c., become by the transposition 21 for 12 made alike through their sums of substitutions.

By
$$H_1^i + H_2^i + H_3^i + H_4^i + H_5^i = {}_1J_i$$
$$H_6^i + H_7^i + H_8^i + H_9^i + H_{10}^i = {}_1K_i$$
$$H_{11}^i + H_{12}^i + H_{13}^i + H_{14}^i + H_{15}^i = {}_1L_i$$
$$H_{16}^i + H_{17}^i + H_{18}^i + H_{19}^i + H_{20}^i = {}_1M_i,$$
$$H_{21}^i + H_{22}^i + H_{23}^i + H_{24}^i + H_{25}^i = {}_2J_i$$

. .
. .
. .
. .

$$H^i_{36}+ \quad . \quad . \quad . \quad . \quad . \quad .={}_2M_i$$
$$\vdots \qquad\qquad\qquad\qquad \vdots$$
$$H^i_{56}+ \quad . \quad . \quad . \quad . \quad +H^i_{60}={}_3M_i,$$

I mean that ${}_1J_i$ is the sum of the ith powers of the functions obtained by effecting, on the subindices of $x_1 x_2 x_3 x_4 x_5$ in H_1, the substitutions written under j_1, ${}_2J_i$ being the sum of the five functions made by operating on H^i_1 by the substitutions under j_2, &c. We form thus 60 values of H^i_1 with the positive, and 60 more with the negative, substitutions.

By
$$ {}_1J^h_i+{}_1K^h_i+{}_1L^h_i+{}_1M^h_i={}_1X_{hi}$$
$$ {}_2J^h_i+{}_2K^h_i+{}_2L^h_i+{}_2M^h_i={}_2X_{hi}$$
$$ {}_3J^h_i+{}_3K^h_i+{}_3L^h_i+{}_3M^h_i={}_3X_{hi}$$

I mean to say that ${}_1X_{hi}$ is the sum of the hth powers of the four functions ${}_1J_i$ &c.

It is evident that H^i_1, H^i_2, &c., can be formed for any integer index i, and as well as ${}_1J^h_i$, &c., for any integer h.

I denote by H^i_{-1}, H^i_{-2}, &c., and by ${}_{-1}J^h_i$, on ${}_{-1}K^h_i$, &c., and by ${}_{-1}X_{hi}$, &c., the results of operating on H^i_1, H^i_2, &c., on ${}_1J^h_i$, &c., and on ${}_1X_{hi}$, &c., by the substitution 2134, effected on the subindices of $x_1 x_2 x_3 x_4 x_5$ throughout the function considered.

It is evident that
$$({}_1X_{hi}+{}_2X_{hi}+{}_3X_{hi})+({}_{-1}X_{hi}+{}_{-2}X_{hi}+{}_{-3}X_{hi})$$
and
$$({}_1X_{hi}+{}_2X_{hi}+{}_3X_{hi})({}_{-1}X_{hi}+{}_{-2}X_{hi}+{}_{-3}X_{hi})$$
are both symmetrical functions of the roots, and rational functions of the coefficients, of $G=0$; as may be readily seen by making $h=i=1$, and observing that the involutions cannot destroy the symmetry in the roots, or the rationality in the coefficients of $G=0$; for the former is the sum, and the latter the product of two functions, either of which becomes the other by any odd number of transpositions of the subindices of $x_1 x_2 x_3 x_4 x_5$, while neither is altered by any even number of such transpositions Wherefore
$${}_1X_{hi}+{}_2X_{hi}+{}_3X_{hi}$$
is the root of a given quadratic, whose coefficients are rational functions of those of $G=0$.

For the very same reason

$$({}_1X_{hi}{}_2X_{hi}+{}_1X_{hi}{}_3X_{hi}+{}_2X_{hi}{}_3X_{hi})+({}_{-1}X_{hi}{}_{-2}X_{hi}+{}_{-1}X_{hi}{}_{-3}X_{hi}+{}_{-2}X_{hi}{}_{-3}X_{hi})$$

and

$$({}_1X_{hi}{}_2X_{hi}+{}_1X_{hi}{}_3X_{hi}+{}_2X_{hi}{}_3X_{hi})({}_{-1}X_{hi}{}_{-2}X_{hi}+{}_{-1}X_{hi}{}_{-3}X_{hi}+{}_{-2}X_{hi}{}_{-3}X_{hi})$$

are rational functions of the coefficients of G=0, and

$${}_1X_{hi}{}_2X_{hi}+{}_1X_{hi}{}_3X_{hi}+{}_2X_{hi}{}_3X_{hi}$$

is the root of such another given quadratic. Also

$${}_1X_{hi}{}_2X_{hi}{}_3X_{hi}+{}_{-1}X_{hi}{}_{-1}X_{hi}{}_{-2}X_{hi}, \text{ and}$$

$${}_1X_{hi}{}_2X_{hi}{}_3X_{hi}{}_{-1}X_{hi}{}_{-2}X_{hi}{}_{-3}X_{hi}$$

are rational in the coefficients of G=0, for the same reason; and

$${}_1X_{hi}{}_2X_{hi}{}_3X_{hi}$$

is the root of such another given quadratic,

We can now form the cubic whose roots are ${}_1X_{hi}$, ${}_2X_{hi}$ and ${}_3X_{hi}$, an equation having for coefficients given irrational functions of the coefficients of G=0, in which every radical has a determined sign. Let $({}_1X_{hi})$, $({}_2X_{hi})$ $({}_3X_{hi})$ be the algebraic roots of this cubic.

By forming and solving this cubic for the values $h=1$, $h=2$, $h=3$, $h=4$, we get, and we are content to get, from each of the four cubics, its one algebraic root $({}_1X_{hi})$, in which there is no ambiguity in the radicals. That is, we obtain

$$\begin{aligned}
({}_1X_i)&={}_1J_i+{}_1K_i+{}_1L_i+{}_1M_i\\
({}_1X_{2i})&={}_1J_i{}^2+{}_1K_i{}^2+{}_1L_i{}^2+{}_1M_i{}^2\\
({}_1X_{3i})&={}_1J_i{}^3+{}_1K_i{}^3+{}_1L_i{}^3+{}_1M_i{}^3\\
({}_1X_{4i})&={}_1J_i{}^4+{}_1K_i{}^4+{}_1L_i{}^4+{}_1M_i{}^4,
\end{aligned}$$

where the left members are all given irrational functions of the coefficients of G=0, in which every radical has a determined sign.

From these equations given for the five values (1, 2, 3, 4, 5) of i, we form easily for every such value the quartic whose roots are ${}_1J_i$, ${}_1K_i$, ${}_1L_i$, and ${}_1M_i$. This quartic will have for coefficients given irrational functions of the coefficients of G=0, every radical having a determinate sign.

Forming now and solving these quartics for the first five values of i, we obtain from each for the one root ${}_1J_i$ an irrational function $({}_1J_i)$ of the given coefficients of the sextic G=0, in which every radical has a determined sign. That is, we obtain

$$({}_1J_1)=H_1+H_2+H_3+H_4+H_5,$$
$$({}_1J_2)=H_1^2+H_2^2+H_3^2+H_4^2+H_5^2,$$
$$({}_1J_3)=H_1^3+H_2^3+H_3^3+H_4^3+H_5^3,$$
$$({}_1J_4)=H_1^4+H_2^4+H_3^4+H_4^4+H_5^4,$$
$$({}_1J_5)=H_1^5+H_2^5+H_3^5+H_4^5+H_5^5,$$

With these data we can form the quintic whose roots are H_1 H_2 H_3 H_4 and H_5. Its coefficients will be given irrational functions of the coefficients of $G=0$ formed from the definite five $({}_1J_i)$, in which every radical will have a determined sign.

Solving this quintic, and substituting its five roots (H_1), (H_2), &c., for H_1 H_2 H_3 H_4, H_5 in our expression for x_0, we have the algebraical solution of the general sextic $G=0$: and every one of x_0 x_1 x_2 x_3 x_4 x_5 is definitely given by the proper reading of the cube roots of unity.

By a like process can every algebraic equation be solved, the nth degree after the $(n-1)$th.

"Description of a Dolerite at Gleaston, in Low Furness," by E. W. Binney, F.R.S., F.G.S.

During the last 30 years the tract of land known as the Hundred of Low Furness has been investigated and described by several geologists. It was one of the earliest fields investigated by the venerable Sedgwick, who has left us a most valuable memoir of his labours in that district. Since then, Mr. Jopling, myself, and Sir R. I. Murchison, and Professor Harkness have published descriptions of the silurian mountain limestone and permian formations of the country. Miss E. Hodgson has also given us information as to the drift deposits overlying the palæozoic strata. Still, notwithstanding what has been done, it may confidently be asserted that the peninsula comprising the southern part of the Hundred of Low Furness has yet to be carefully examined before its geology can be said to be thoroughly known.

None of the above-named persons appear to have been aware of the occurrence of any trap dykes in this district,

judging from their published writings, with the exception of Mr. Jopling, who, in his sketch of the geology of Low Furness and Cartmel, comprehending the Hundred of Lonsdale north of the sands, published in 1843, when speaking of the geology of Gleaston, says:—"Carboniferous limestone abounds, and in the quarries near the castle are many fossils beautifully preserved in the shale beds between those of the limestone; there is also a vein of trap." At page 72, the same author says "there are also appearances of trap near Gleaston, associated with limestone breccia."

In the month of October last, Miss E. Hodgson was so kind as to send me some specimens of rocks from Gleaston, which puzzled her a good deal. Some of the parties to whom she had sent them called them dolomites, whilst others named them traps and greenstones. To the latter opinion Miss Hodgson, I believe, was inclined to add the weight of her sanction. Not having previously seen, or even heard of, the occurrence of any such rocks in the district where they were said to be met with, I went over to examine them, and having been furnished with information by Miss Hodgson, easily found the place where they are exposed at Gleaston Green. At that time Mr. Jopling's book had not been seen by me. The space occupied by these singular rocks, at least so far as at present exposed, is so limited that all that can be seen is very soon ascertained. Specimens were collected, and a few observations made. The former, by the kindness of my friend Professor Roscoe, F.R.S., were analyzed for me in the laboratory of Owens College. It is only by the labours of the chemist that geologists can with any certainty decide upon the age and origin of such rocks as those which are met with at Gleaston.

On approaching Gleaston Green from Scales, the mountain limestone appears to occupy the country so far as it can be seen. In a quarry below the old castle on the roadside, this rock in the northern part is very hard, and dips

to the west at an angle of 25°, whilst in the southern part, where it is softer, it dips in the same direction at an angle of 16°. Owing to the covering of drift, the limestone is not seen nearer to the mill, but it probably extends further in that direction. At a short distance below the mill, dark-coloured laminated shales are seen in the bank on the roadside, dipping apparently at an angle to the N.N.W. We then come to the rocks at the end of the Green. They appear to run in an east and west direction, and are not now exposed for more than twenty yards. From north to south they may probably extend about forty yards, but certainly for more than half of that distance, towards the beck, they are not now seen until the land rises on the bluff south of the beck, where they reappear as a reddish and bedded trap ash, having an east and west direction, and dipping N.N.W. at an angle of about 60°. This ash is succeeded by a coarse breccia of a few yards in thickness, so far as exposed, which dips slightly north of west, at an angle of 25°, and then is covered up by grass so as not to be seen, but no doubt, from sections in the adjoining lane and borings made on the rise of the strata, dark-coloured shales occur again, and the dyke most probably intrudes through these shales, which are in every respect like limestone shales, but no organic remains were observed in them so as to make us certain of their geological age.

Returning to the north side of the beck, nothing is exposed of the district west of the hard rocks seen on the Green, owing to the thick covering of drift in that direction; but Mr. Hodgson has proved, by a series of bore-holes, the occurrence of upper permian sandstone, red shale, and limestone shale — the first to the S.W., the second to the west, and the third to the N.W. of Gleaston; and Mr. Ashburner has proved limestone shale and limestone in bores to the E. and N.E. of the locality where the rock is found.

In the bluff on the south side of the stream, as previously stated, the rock appears more like a trap ash of a reddish-brown colour, and exhibits traces of bedding and white lines like carbonate of lime. Immediately adjoining trap ash, and on its rise, occurs the coarse breccia composed of fine-grained siliceous rocks, cemented together with quartz, and like a permian breccia; but although the beds are near together, there was not evidence to show whether the trap ash gradually passed into the breccia or intruded through it, still the breccia appeared to dip in the same direction, but at a much less angle, namely, 25° to a little west of north. This is a very interesting fact to prove; for if the rock graduates into the breccia, it would appear to be of permian age, and most probably a melaphyr, but if it is intrusive, as the evidence on the whole appears to prove, all we can say is that it is of later date.

This breccia is composed of angular pieces of a fine siliceous stone, of a pink colour, more resembling quartzite than anything else cemented together by small quartz crystals, and containing minute quantities of protoxide of manganese. The form of the fragments is very like that of the rocks in the permian breccia of Rougham Point, near the mountain limestone of Humfray Head, and there consisting for the most part of the neighbouring mountain limestone, but no limestone has yet been met with in the Gleaston breccia as might have been reasonably expected; and the pink quartzite is a rock hitherto unknown in the district. The permian breccia so far as my experience goes, although sometimes containing volcanic ash are composed of the rocks now found in the neighbourhood where they occur, and nearly always vary with the older geological rocks of the district. The composition of the Gleaston breccia makes me hesitate in designating it as permian as it may be some rock altered by the dyke.

The rock in its best state of preservation is remarkably

hard, of a reddish brown colour, has a moderately straight fracture, and a pinkish white streak, and its specific gravity is 2·92.

Three average samples of the rock were taken, two from the north and one from the south side of the beck. All of them were more or less decomposed by exposure to air and moisture, but No. 26 much less than Nos. 23 and 24. Professor Roscoe on analysis, found their present chemical composition to be as follows:—

	South of Stream, No. 23.	North of Stream, No. 24.	North of Stream, No. 26.
Silica	45·54	50·96	51·10
Peroxide of Iron	24·76	24·20	21·58
Alumnia	7·70	14·48	9·40
Lime	13·84	7·32	6·24
Magnesia	0·57	0·55	1·33
Carbonic Acid	2·78	1·90	2·70
Alkalies, Water (by difference)	4·82	0·59	7·65
	100·00	100·00	100·00

The only rock which I know of a similar composition, is a probable variety of green earth, resembling a decomposed pyroxene, described by Macfarlane, in the Canadian Naturalist, New Series, vol. iii., No. 1, page 5, in a paper on the cupriferous bed of Portage, Lake Michigan, which consists of—

Silica	46·48
Alumnia	17·71
Protoxide of Iron	21·17
Lime	9·89
Magnesia trace	—
Alkalies (by difference)	1·97
Water	2·78

Mr. David Forbes, F.R.S., to whom were forwarded small specimens of the rocks and the above analyses, kindly informed me that the rocks were so much decomposed that it was difficult to pronounce with certainty as to what they

were, but he was inclined to think that they were an intrusive dolerite of carboniferous age rather than a melaphyr. The iron had been changed from a protoxide into a peroxide, and the lime had resulted from the decomposition of a lime felspar. As Mr. Forbes had found the presence of titanium united with iron in all the carboniferous dolerites he had examined; I took several ounces of the three samples above given, and having heated them in a crucible so as to convert the iron from a per into a protoxide, and extracted it by a magnet. About half an ounce of this iron was very carefully examined by Mr. Thorpe in Dr. Roscoe's laboratory, especially for titanic acid, and no trace of that substance was found. He used the test with microcosmic salt, having separated iron and silica. The absence of titanium in the rock would lead us to believe that it was of later origin than the carboniferous age, but if traces of that metal had been found, it would not only have settled the question as to the age, but it would have shewn a connexion with the hæmatite iron ores of Whitehaven and Ulverston, all of which contain more or less of titanium as proved by the deposits of that metal found on the sides of old furnaces where hæmatite has been smelted. Is the rock of permian age? It is certainly not much unlike the melaphyr of the German Geologists, and the breccia near the dolerite is not greatly different from that of Ballochmyle, described by Mr. A. Gekie, F.R.S., in the Geological Magazine, for December, 1866, but we could not obtain direct evidence that the breccia gradually passed into the trap, the latter appeared to protrude through it, but certainly the trap and the breccia dipped in the same direction, the one at about 60° and the other at 25° a little West of North. This point can only be satisfactorily determined by cutting a trench and shewing the contact of the breccia with the trap. The extent of the dyke can only be traced for a few yards east and west as previously stated, and none of the

hæmatite iron deposits, so far as known, have been found south of it. Its age also appears to be more recent, even supposing it to be permian, than those deposits, which for the most part must be considered of carboniferous age. The occurrence of this trap might have been considered to have some connection with the deposition of the iron had it been of carboniferous age, but it is evidently more recent, and therefore could have had nothing to do with it further than disturb or displace it.

"A Search for Solid Bodies in the Atmosphere," by R. Angus Smith, Ph.D., F.R.S., &c.

I have so frequently for many years attempted to find, and have found, organic substances which have passed from the air into liquids in which they were collected, that perhaps the Society will scarcely attend to another attempt, although it indicates, I think, some progress. It was in the year 1847 that I first collected what I believe was matter from the respiration and perspiration, and found that as it was kept it grew into distinct confirmed forms.

Whilst examining some matters relating to the cattle plague I found one or two remarkable points. I had before that time used aspirators to pass the air through liquids, except in the oxidation experiments. At that time I used simply a bottle which contained a little water. The bottle was filled with the air of the place and the water shaken in it. The difference of air was remarkable. A very few repetitions would cause the liquid to be muddy, and the particles found in many places were distinctly organic.

Lately I tried the same plan on a larger scale. A bottle of the capacity of $\frac{2}{1000}$ c.c. was filled with air and shaken with water. The bottle was again filled and shaken with the same water, and this was repeated 500 times, nearly equal to $2\frac{1}{2}$ million cbc., or 2,495 litres. As this could not be done in a short time, there was considerable variety of

weather, but chiefly dry, with a westerly wind. The operation was conducted behind my laboratory, in the neighbourhood of places not very clear, it is true, but from which the wind was blowing to all parts of the town. I did not observe any dust blowing, but if there were dust, it was such as we may be called on to breathe. The liquid was clouded, and the unaided eye could perceive that particles, very light, were floating. When examined by a microscope the scene was varied in a very high degree—there was evidently organic life. I thought it better to carry the whole to Mr. Dancer and to leave him to do the rest, as my knowledge of microscopic forms is so trifling compared to his. It may, however, interest the Society to hear of a few of these previous attempts, the latest made till recently. I shall therefore read from a report to be found in the appendix to that on the cattle plague.

Mr. Crookes also brought me some cotton through which air from an infected place had passed. It was examined at the same time. Taking cotton in the mass nothing decided was seen; but when it was washed some of the separate films were coated over with small nearly round bodies, presenting no structure, or at least only feeble traces of it, and perhaps to be called cells. I had not sent gun-cotton, as I intended, to Mr. Crookes, fearing the rules of the post; otherwise there would have been more certainty that the bodies spoken of did not exist previously on the cotton. However, Mr. Dancer, who has examined cotton with the microscope oftener than most persons, even of those experienced in the subject, had never observed a similar appearance.

The liquid had also a number of similar bodies floating in it.

It was then that Mr. Crookes sent a liquid which he had condensed from the air of an infected cowshed at a space a little above the head of a diseased cow. It was also examined, and it presented similar indications of very numerous

small bodies. Not being a professed microscopist I shall not attempt a description, but add that they clearly belonged to the organic world, and were not in all cases mere debris. We found also one body a good deal larger than the rest; it resembled somewhat a paramecium, although clearly not one.

We found no motion whatever, and only this latter substance could be adduced as an absolute proof of anything organized being present. Next day I examined the same liquid; and, whether from the fact of time being given for development or from other causes, there was a very abundant motion. There were at least six specimens in the field at a time, of a body resembling the euglena, although smaller than I have seen it. When these minute bodies occur it is clear that more may exist, and germs in this early stage are too indefinite to be described. The existence of vital sparks in the organic substances in the air alluded to is all I wish to assert, confirming by a different method the observations of others. It might, of course, be said that since the bottle was opened at Mr. Dancer's the air at that place may have communicated them. I answer that, before it was opened, a good glass could detect floating matter, some of it, however, as in the microscope proved, indefinite enough.

Finding this, and fearing that the long time needful to collect liquid from the atmosphere might expose it also to much dust, I used a bottle of about 100 cubic inches dimensions, and putting with it a very little water, not above five cub. centimetres, I pumped out the air of the bottle, allowing the air of the place to enter. This was done six times for each sample, the water shaken each time, and the result examined. This was done with the same bottle that was used in my early experiments with permanganate, and by the same method, except that water instead of that salt was used. At first considerable numbers of moving particles were found; but it was needful to examine the water used, and here occurred a difficulty. It

was not until we had carefully treated with chemicals and then distilled the water again and again that we could trust it. Particles seemed to rise with the vapour, and if so, why not with the evaporating water of impure places.

Having kept an assistant at the work for a week, and having myself examined the air of three cow-houses, I came to the conclusion that the air of cow-houses and stables is to be recognized as containing more particles than the air of the street in which my laboratory is, and of the room in which I sit, and that it contains minute bodies, which sometimes move, if not at first, yet after a time, even if the bottle has not been opened in the interval. There is found in reality a considerable mass of debris with hairs or fine fibres, which even the eye, or at least a good pocket lens, can detect. After making about two dozen trials, we have not been able to obtain it otherwise. Even in the quiet office at the laboratory there seemed some indications.

I found similar indications in a cow-house with healthy cows; so I do not pretend to have distinguished the poison of Cattle Plague in these forms; but it is clear that where these exist there may be room for any ferment or fomites of disease; and I do not doubt that one class is the poison itself in its earliest stage. It would be interesting to develope it farther.

I have recorded elsewhere that I condensed the liquid from the air of a flower garden, and found in it, or imagined I found, the smell of flowers. I do not remember that I looked much to the solid or floating particles, thinking them to be blown from the ground, but it does not affect the result, whether they be found constantly in the air or are raised by the action of currents.

"Microscopical Examination of the Solid Particles from the Air of Manchester," by J. B. DANCER, F.R.A.S.

The air had been washed in distilled water, and the solid

matter which subsided was collected in a small stoppered bottle, and on the 13th of this month Dr. Smith requested me to examine the matter contained in this water. An illness prevented me from giving it so much attention as I could have wished.

The water containing this air-washing was first examined with a power of 50 diameters only, for the purpose of getting a general knowledge of its contents; afterwards magnifying powers varying from 120 to 1,600 diameters were employed.

During the first observations, few living organisms were noticed; but, as it afterwards proved, the germs of plant and animal life (probably in a dormant condition) were present.

I will now endeavour to describe the objects found in this matter, and begin in the order in which they appeared most abundant.

1st. *Fungoid Matter.*—Spores or sporidiæ appeared in numbers, and, to ascertain as nearly as possible the numerical proportion of these minute bodies in a single drop of the fluid, the contents of the bottle were well shaken, and then one drop was taken up with a pipette; this was spread out by compression to a circle $\frac{1}{2}$ an inch in diameter. A magnifying power was then employed, which gave a field of view of an area exactly 100th of an inch in diameter, and it was found that more than 100 spores were contained in this space; consequently the average number of spores in a single drop would be 250,000. These spores varied from 10,000th to 50,000th of an inch in diameter. The peculiar molecular motion in the spores was observable for a short time, until they settled on to the bottom of the glass plate; they then became motionless.

The Mycelium of these minute fungi were similar to that of rust or mildew (as it is commonly named), such as is found on straw or decaying vegetation.

When the bottle had remained for 36 hours in a room at

a temperature of 60° the quantity of fungi had visibly increased and the delicate mycelial thread-like roots, had completely entangled the fibrous objects contained in the bottle and formed them into a mass.

On the third day a number of ciliated zoospores were observed moving freely amongst the sporidiæ. I could not detect any great variety of fungi in the contents of the bottle, but I cannot presume to say that all the visible spores belonged to one species, and as there are more than 2,000 different kinds of fungi it is possible that spores of other species might be present, but not under conditions favourable for their development. Some very pretty chain-like threads of conidia were visible in some of the examinations.

The next in quantity is vegetable tissue. Some of this formed a very interesting object, with a high power, and the greater portion exhibited what is called pitted structure. The larger particles of this had evidently been partially burnt and quite brown in colour, and were from coniferous plants, showing with great distinctness the broad marginal bands surrounding the pits; others had reticulations small in diameter. They reminded me of perforated particles so abundant in some kinds of coal.

The brown or charred objects were probably particles of partially burnt wood used in lighting fires.

Along with these reticulated objects were fragments of vegetation, resembling in structure hay and straw and hay seeds, and some extremely thin and transparent tissue showing no structure. These were doubtless some portions of weather-worn vegetation. A few hairs of leaves of plants and fibres, similar in appearance to flax, were seen, and as might have been expected in this city, cotton filaments, some white, others coloured, were numerous; red and blue being the predominant colours. A few granules of starch, seen by the aid of the polariscope, and several long elliptical bodies, similar to the pollen of the lily, were

noticed. After this dust from the atmosphere had been kept quiet for three or four days, animalculæ made their appearance in considerable numbers, the monads being the most numerous. Amongst these were noticed some comparatively large specimens of paramecium aurelia, in company with some very active rotiferæ; but after a few days the animal life rapidly decreased, and in twelve days no animalculæ could be detected.

Hairs of Animals.—Very few of these were noticed, with the exception of wool; of this both white and coloured specimens were mixed up along with the filaments of cotton.

After each examination as much of the drop of water as could be collected by the pipette was returned to the bottle, in order to ascertain if any new development of animal or vegetable life would take place, and the stopper of the bottle was replaced as quickly as possible to prevent the admission of the particles from the air in the room; and I am tolerably certain that the objects named in this paper are those which the bottle contained when Dr. Smith brought it to me.

The particles floating in the atmosphere will differ in character according to the season of the year, the direction of the wind, and the locality in which they are collected, and, as might be expected, are much less in quantity after rain.

The small amount of fluid now remaining in the bottle emits the peculiar odour of mildew, and at present the fungoid matter appears inactive.

For the purpose of obtaining a rough approximation of the number of spores, or germs of organic matter contained in the fluid received from Dr. Smith, I measured a quantity by the pipette, and found it contained 150 drops of the size used in each examination. Now, I have previously stated that in each drop there were about 250,000 of these spores, and as there were 150 drops, the sum total reaches the startling number of 37½ millions, and these, exclusive of

other substances, were collected from 2·495 litres of the air of this city*—a quantity which would be respired in about 10 hours by a man of ordinary size when actively employed. I have to add that there was a marked absence of particles of carbon amongst the collected matter.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 24th, 1868.

J. B. DANCER, F.R.A.S., President of the Section, in the Chair.

Mr. SIDEBOTHAM sent two beautifully finished water-colour drawings, accompanied by the following note:—

"I send you a couple of drawings of the dry-rot fungus *Merulius lachrymans,* remarkably fine. Mr. Lynde, our Treasurer, brought me the specimens, which he had met with during the demolition of some old buildings. The drawings are of the natural size. It is very rarely that such perfect specimens are found, showing, as this one does, the curious structure of the species, and its connection with both the pore and the gill-bearing divisions of fungi."

Mr. DANCER exhibited some sand from the sea-shore at Santos, South America. This sand was remarkably silvery in appearance, a large portion of it consisting of minute plates of mica, and very transparent fragments of quartz, which made it an interesting object for the polariscope. It was rich also in Foraminifera, spines of Spatangus, and fragments of Coralline.

* Behind Dr. R. Angus Smith's laboratory.

"Remarks on Molecular Activity as shown under the Microscope," by J. B. DANCER, F.R.A.S.

The Author stated that, during the last 30 years, he had met with many microscopical observers who were not acquainted with the phenomenon, to which the name of molecular action has been given. This class of microscopists had confined their attention to objects requiring a very moderate amount of magnifying power, and generally to dry objects; but when their investigations extended to minute objects immersed in fluid which required powers of 800 to 1500 diameters, they were startled by the appearance of particles in active motion, not moving in a direct line, but vibrating as if attracted and then repelled by each other, some single, and others in clusters.

Many instances have come under the author's notice, in which these objects have been regarded by microscopists as animalculæ. They have given rise to many very ingenious speculations, some of which are connected with spontaneous generation, these observers would have been saved much labour if they had been acquaintad with the experiments of the late Dr. Robert Brown on active molecules.

The author does not imagine that the members of this section are wholly unacquainted with the experiments of the early microscopists on this subject, but in the absence of more important matter, he thinks a brief account of the early observations on these so called active molecules may interest them.

The moving particles had been noticed by Leewenhock, Stephen Gray, Buffon, and others who supposed them to be animated matter.

In the year 1827, the late Dr. Robert Brown, whilst engaged in the microscopical investigation of unimpregnated ovulum, noticed that the pollen of the clarckia pulchella was filled with particles, which appeared in active motion when immersed in water. These observations were followed by

the examination of the pollen of other plants, the particles of which he found to exhibit similar activity. For some time he was exceedingly perplexed with these phenomena, and was disposed to believe that he had really seen in these minute bodies the supposed constituents or elementary molecules of organic bodies, first so considered by Buffon, Wrisberg, Muller, and Milne Edwards, and on examining various animal and vegetable tissues, whether living or dead, he found, as he had expected, that active molecules were visible by merely bruising the substances in water.

Continuing his observations, he found that particles from a bruised specimen of fossil wood appeared to consist entirely of these moving bodies. From this he inferred that these molecules were not limited to organic bodies, or even to their products.

After this he proceeded to examine minerals, simple earths, metals, and many other substance too numerous to mention, and with similar results.

Some writers who commented on these experiments, but who had not carefully followed his communications, asserted that Dr. Brown imagined these particles to be animated,—and this statement was generally believed.

In 1829 the author's late father repeated many of Dr. Brown's experiments, and to prove that these moving particles could not be animalculæ, he placed some crystals and minerals in a crucible which he subjected to a red heat, ground portions of them to powder, then put it into distilled water, and shewed the particles in motion to his scientific friends. It is now well known that all kinds of matter, if reduced to sufficiently small particles, and placed in a medium in which they will not readily sink, will exhibit these movements.

Now, as this phenomenon occurs in the cells of plants, the yolk of the egg, and in decomposing animal and vegetable matter, it is not surprising that the early microscopists, and,

indeed, modern ones, should have mistaken them for animalculæ. The particles which exhibit the greatest activity are exceedingly minute, ranging from 10,000th to 30,000th of an inch in diameter; they remain active a considerable time if they are nearly of the same specific gravity as the solution in which they are immersed. One simple mode of producing them is to rub a little gamboge in water, on a glass slide, and place a thin glass cover on it, using a power of from 800 to 1,200 diameters. They can easily be distinguished with less magnifying power, but are not so effectually shown. If they are required for prolonged examination, Dr. Brown recommends that the solution of gamboge be mixed with a little almond oil. The minute globules of water are thus surrounded by oil, and rapid evaporation is prevented.

The cause of the phenomenon is not yet satisfactorily accounted for. Some have imagined that it is the physical repulsion of the particles when uninfluenced by gravitation. The author has tried many experiments with electricity and magnetism without success. He thinks that the movement may possibly be connected with the absorption and radiation of heat.

Those interested in Dr. Brown's experiments and observations on active molecules, can refer to his republished papers in Vol. I. of the Ray Society's publications for 1866.

After the meeting, slides containing active molecules were exhibited to the members.

A conversation took place on the preservation of dried plants, in which Mr. Bailey stated that he uses with success glue with carbolic acid for attaching them to the paper, as a preservation against mites, placing also a few crystals of the dry acid in his cabinet.

Ordinary Meeting, April 14th, 1868.

R. Angus Smith, Ph.D., F.R.S., Vice-President, in the Chair.

Mr. Wm. Brockbank and Mr. A. Brothers were appointed Auditors of the Treasurer's Accounts.

Mr. S. Broughton said that, on recently observing with high powers a group of fine spots on the sun, one of which was of considerable magnitude, it occurred to him to remove the dark glass, and by keeping the eye much beyond the focus of the heating rays, and at such a distance that the spot almost filled the apparent field of the eye-piece, to see if any phenomena could be observed different from those seen through the dark glass.

The spot was at once seen to be of a dark blood red; but, thinking this perhaps might be from the strong contrast of colour, he attached a disc of plaster of Paris to the telescope, and projected the image of the spot on it. On looking at this with a common pocket magnifier, the image was observed to be a dark blood red, although the observatory was not darkened, and the disc merely shielded from the direct rays by an intervening opaque substance.

If these observations are confirmed, it will corroborate the opinion long held that the spots are not black, but appear so by contrast, and, as it would seem, from the intervention of the coloured glass.

"Brief Notes on the Mutations of Imponderable Elements," by J. C. Dyer, Esq., V.P.

In the history of speculative science we have seen a great many learned and ingenious theories to account for the

varied phenomena presented by the elements that evade the direct tests of weight and measure—such as light, electricity, and magnetism, and more especially that called heat or caloric. With respect to the nature of the latter, directly conflicting views have been taken by philosophers of eminence, both ancient and modern, in expounding the causes of calorific phenomena,—on the one hand denying and on the other affirming the identity or reality of heat as a natural element. Among the ancient asserters of the nonentity doctrine, we find the names of Boyle and his followers; and in modern times those of Count Rumford, Sir Humphry Davy, Dr. Joule, and many others of less eminence: and in support of the material nature of heat, we have the names of Boerhaave and his school, and in later times those of Dr. Black, James Watt, Sir James Leslie, Dr. Hulton, Dr. William Henry, and Dr. Dalton, with others of note. Without here discussing the diverse grounds of difference on the questions at issue (having elsewhere dwelt at length on their respective bearings) I proceed to explain those on which the materiality of heat appears to rest, and whereon the entire range of phenomena connected with the imponderable elements may admit of rational solutions.

I assume the existence of four distinct states or conditions of heat, and which are thus defined, viz.—(1) elemental or neutral heat, (2) latent or combined heat, (3) sensible or expanding heat, and (4) radiating heat. Of the two last forms little need be said beyond what is directly seen in their alternate changes into and out of the other forms of heat (explained further on). The existence of a pervading medium in space has been admitted by the most eminent physicists; but no explanations are given of its nature, except that of its elasticity, nor yet of what are the relations of this medium to those of light, heat, electricity, and magnetism. It is treated as a pervading "ether," or "luminous ether," and its resistance to the transit of light

through space has been estimated and given in figures, by a profound philosopher, who, in treating of the undulations of light, describes it as "an elastic medium pervading all space, not even excepting what is occupied by what we call material bodies, that is, such as have weight and which to us constitute the visible and tangible universe of things."—(Sir John Herschel, Bart., "On Light," in *Good Words*, for July, 1865.)

In the works of Boerhaave the said medium is described as "a calorific element, *sui generis*, pervading all space and all bodies, and being in itself of the nature of fire." Thus "a pervading element" rests on very high authority, both ancient and modern; and it is of the nature of such element, and its agency in natural phenomena, that I propose to treat, and to show that the different forms of imponderable matter, as light, heat, electricity, and magnetism, are all essentially identical, and in proof of this we must look to their reciprocal interchanges with each other, whereby their common origin, in one pervading element, will be made probable, if not a physical certainty.

The term formerly applied to this pervading element was a "calid" or "calorific" element,—thus conveying the idea of active fire or very great heat; but they do not accord with the nature or properties which I ascribe to such pervading element, that of "neutral or elemental heat;" and the term latterly employed to indicate the same element is also inapplicable, since it only points to one of its properties, viz., elasticity. I have, therefore, adopted the term of "neutral heat," which is meant to convey the idea or notion that this element, in fact, constitutes the source, or fountain, whence all other forms of heat are derived, and into which they alternately return. This neutral element is everywhere of equal density, by reason of its elastic tension, and it therefore cannot exert or possess the force of gravity or of inertia, and it must remain in a quiescent state, except in cases

where its equilibrium becomes disturbed by external forces, mechanical or chemical, as after mentioned.

Let us consider the nature of these forces, and, first, the vast mechanical forces continually acting upon this medium by the earth's orbital passage through it. As the earth advances her force will compress the medium in front of her path, but its equilibrium must be instantly restored by its reaction in her wake. This reaction explains the slight retarding force of the medium on the periodic motions of the planets. Now, the disturbing forces of these orbital movements, along with the axial rotation of the earth, serve to generate continuous currents in the medium, and which constitute what we call magnetism, or the magnetic currents, such as flow from the central action in the earth's path towards the north and south poles. In addition to the invisible currents, shown by the magnetic phenomena, we may fairly assume that this mechanical action of the earth generates those luminous currents in the medium that are known, at certain seasons, as ground lightning, and at others as auroras or corruscations of polar lights.

We have next to consider the frictional action of the surfaces of the sun and earth upon that portion of the medium which extends in a right line between the sun and the earth, and which, acting at the ends of the intervening column, excites the medium into those undulations or vibrations that act on our senses, and give the impression of white light when seen through clear air, and of coloured light when the undulations are disturbed by clouds or vapours. But, to receive the impressions, we must be within the intervening column, or near it, as in twilight. Now, although this medium is only perceptible as light, yet the medium itself is at all times present, equally in the night and in dark rooms as in daylight,—for when its action as light is cut off by opaque bodies, still the ambient element continues to act on our feelings and bodies as sensible heat, and this in different

degrees of temperature, and when the heat is intense it becomes visible as red, or white heat and light. The duplex nature of light, too, is shown by the action of the prism. In the spectrum, where an increase of heat appears by moving the thermometer from the lightest to the darkest or red rays, and a still higher temperature is shown when the bulb is placed just outside of the visible spectrum, viz., in the dark rays. Thus clearly proving that, by this mechanical action on transmitted light among the visible rays, it is partially converted into heat, and wholly so converted outside of the spectrum or in the dark rays. We find also that sensible heat obeys the same laws of reflection and refraction that govern the motions of light, and differs only in the transmission through opaque or transparent bodies. Besides, we see the mechanical action of the lens gives direct proof of the conversion of light into heat by the simple convergence of its rays upon a point, whereby intense heat is produced. The plain inference, then, is that an element thus alternately exhibiting both the mingled and separate phenomena of light and heat, must possess identical properties, and that those several conditions of it are to be ascribed to the mutations of one element when subjected to the said acting forces. The facts adduced are clearly explained on this assumption, viz.—of the identity of light and heat, whilst they present a strange mass of anomalies, if viewed as produced by the movements of two distinct elements in space.

I pass from mechanical action to consider the chemical forces, continually acting on and by this medium, whereby its mutations are effected, and among these, the most extensive are caused, by the affinities between heat and water, as shown in common surface evaporation. Without here dwelling upon the diverse doctrines or views, of late entertained, respecting the nature of latent or specific heat, it will suffice to state the following facts, viz.—that 212° of

sensible heat are required to raise water to the boiling point, under the atmosphere, and then to convert the water into steam, 940° more, sensible heat, are absorbed—this latter disappears, as sensible heat, (call it latent, specific, or otherwise) but when the steam is condensed and returns to water, then the same 940° reappears as sensible heat, Now we all know that such absorption and redischarge of heat is continually taking place, in the evaporation from the earth's surface, by the action of solar heat. This heat then to the extent, at least, of 940° becomes latent in the steam, or invisible aqueous vapour, at the surface, and the same heat is again given out in the upper air, where the vapour is condensed into clouds or fogs. The experiments and estimates of Dr. Halley, enable us in some degree to perceive the stupendous scale of the daily ascent of water in vapour,* into the higher regions of the air, when its heat, both sensible and latent, is evolved. But this heat, does not thence return to the earth, as sensible heat, because the temperature above the clouds is always at or near the freezing point. What then becomes of these vast floods of sensible heat, that enter the vapour at the surface and entirely disappear in those cold regions above ? The plain answer seems to be that the entire heat, so ascending and disappearing, simply passes into its normal, or elemental form, of neutral heat. On this view of the mutations of heat, we see why it must become more or less redundant in the upper air, according to the varying rates of evaporation. These mutations of heat, will account for its becoming redundant, or of higher elastic tension, as a neutral element,

* Dr. Halley (in the "Philosophical Transactions") "has shown that more than five thousand millions of tons of water, ascend, in vapour, daily, from the surface of the Mediteranean only." Now it follows that a mighty force must be exerted in raising this stupendous weight of water, (to the height of some thousand feet) against the force of gravity, in the water, and the elastic force of pressure by the air. But this evaporating force is solely due " to the known relations of heat and water," and is (as before said) a chemical force.

in the upper air, and comparatively deficient near the earth, whereby the balance of tension, or its equilibrium, must become more or less disturbed according as evaporation varies at the surface. In this way such continuous fluxions of heat into the higher regions, and its redundant state there, generates and sustains the return currents of the element from above towards the lower clouds and the earth, which elemental movements constitute the electrical currents, such as are shown by electrometers. The diurnal variations of these currents arise from the differing rates of evaporation in the day and night. The ordinary course of these elemental fluxions apply to the common invisible currents, but, in like manner, when the mutations are excessive, they will account for the more rapid and violent electrical phenomena, constituting the thunder storms and lightnings, which so often occur within the tropics, and sometimes in our latitudes when evaporation is very rapid.* In fine, all of the corruscations, or vivid transits of the said element to restore its disturbed equilibrium are to be taken as constituting what we call the electrical and magnetic currents—as before said. Since we find that mechanical forces are exerted, as in telegraphy, &c., both by the electrical and magnetic currents, their materiality is clearly proved, for such action can only be by contact of natural bodies; and by conjoint action they are shown to be merely two conditions of one and the same element, just as the mingled and alternate action of light and heat show them to be but two forms of one element. The amounts of latent heat in different bodies may be here passed by, but one other case of latent heat in aqueous

* "The phenomena of aqueous meteors, such as rain, fog, and dew depend upon the known relations of heat and water; electricity appears to be a consequent, rather than an agent, in the formation and decomposition of clouds, or if a necessary agent, it is equally so in the boiling of water in our houses or the drying of piece goods in a stove."—Dalton: *Memoirs Literary and Philosophical Society*, vol. 4, 2nd series, page 123.

vapour should be noticed, viz. the heat evolved in respiration. Beneficent nature always provides for her own wants in excess. The carbonic acid formed in the lungs gives out the needful heat to keep up the temperature of the warm-blooded animals, and the excess of heat so evolved is taken up by the water converted into steam and which is carried away in breathing. Now, if the whole of the heat so absorbed in the steam were to remain as sensible heat in it, the 940° held latent therein would at once produce a temperature quite destructive of the living textures and would of course extinguish life. Thus we see another of the sublime laws of Providence is made to rest on these mutatations of the imponderable elements. Little need here be said concerning radiating heat as apart from that of light, yet one instance may be cited, viz., the experiment of suspending a thermometer from the top of a glass-receiver on the table of an air-pump, then by rapidly drawing out the air the temperature will fall to or near the freezing point, but the mercury will again slowly rise till it reaches the temperature of the room; but as the thermometer (attached by a non-conductor of heat) is quite isolated in a vacuum, how can the heat reach and re-enter the bulb, except it be by radiating from the external warm air through the glass and the vacuum, and thus restoring the equal temperature inside and outside of the receiver. This process is of the same kind as that of heat radiating from a common fire-place to warm a room.

In my "Notes on Heat in Relation to Atmospherical phenomena," read at the Society, February 9th, 1853, besides adducing reason for assuming that all electrical and magnetical phenomena are simple consequences of the mutations of one pervading element, I have therein shown how most of the great aerial and oceanic currents may be traced to the acting forces generated by the mutations of heat, when they are excited in conjunction with those of gravity.

Now, since all philosophers recognise the presence everywhere of the three elements of light, electricity, and magnetism, as also the fact of their reciprocal interchanges of form, as exhibited in phenomena; and as we find, also, the general admission of "an elastic ether" in space, it would seem far more in harmony with "the simplicity of nature's laws" to ascribe those known forms of "the imponderable elements" to the mutations of one than to the existence, in space, of four distinct elements. I have accordingly aimed to show that each of the known conditions of imponderable matter are traceable to the mutations of the one pervading element, and, for the reasons before stated, I call this elemental or neutral heat.

"Researches on Di-Methyl," by Wm. H. Darling, Dalton Scholar in the Laboratory of Owens College. Communicated by Professor H. E. Roscoe, Ph.D., F.R.S.

The synthesis of carbon compounds forms perhaps the most important and interesting branch of modern chemical enquiry. The most recent developments of these synthetical processes are the now well ascertained facts of the dependence of the chemical properties of the molecule, upon the position of the individual atoms of which that molecule is built up.

Any isomeric modifications of the saturated monovalent compounds containing one or two atoms of carbon can only be explained by the existence of a difference between the four combining powers of each carbon atom, whilst in the tri-carbon and higher series isomerism indicates the difference in the power of combination existing between the end and the middle carbon atoms of the chain.

From Frankland's original observations concerning the difference between the action of chlorine on the so-called di-methyl $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}$ obtained by the electrolysis of an alkaline

acetate, and on the hydride of ethyl $\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}$, obtained from ethyl compounds, the existence of a difference in the four combining powers of a carbon atom was rendered probable.

The subsequent researches of Schorlemmer have, however, proved that only one hydrocarbon of the formula C_2H_6 exists, inasmuch as he succeeded in preparing ethyl chloride from the hydrocarbon di-methyl $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}$, obtained by the electrolytic decomposition of an alkaline acetate (*Proc. R. Soc.*, XIII., 225); as well as from ethyl hydride, obtained from ethyl compounds.—(*J. Chem. Soc.*, N.S., II. 262.)

It appeared of great interest to repeat this synthesis, and to prepare the chloride in larger quantity, from which to prepare ethyl compounds and ascertain their chemical and physical properties.

At the request of Mr. Schorlemmer, I undertook this investigation. I take this opportunity to express my thanks to that gentleman, and also to Professor Roscoe for the kind assistance rendered to me throughout this research.

I prepared the di-methyl by the electrolytic decomposition of acetate of potash, according to the process described by Kolbe. The gas, evolved from a platinum plate contained in a porous cell, was passed first through a solution of caustic potash, to absorb the carbonic acid, afterwards through nordhausen acid, and over pumice stone moistened with oil of vitriol, to free it from a trace of oxide of methyl or hydrocarbon absorbable by this acid, and finally through a solution of caustic potash, to absorb acid fumes, any carbonic acid which had escaped the first wash bottle, or traces of sulphurous acid. The gas thus prepared had a very slight odour, it burnt with a non-luminous flame.

The following analysis of it is according to Bunsen's method :—

	Volume.	Pressure.	Tempt. C.	Volume at 0° C and 1m. Pressure.
(1.) Original volume of Gas (moist)	183·48	0·1402	8·5	24·94
(2.) After addition of oxygen ,,	333·41	0·3919	8·0	127·00
(3.) ,, ,, air ,,	567·52	0·5221	8·5	287·5
(4.) ,, explosion ,,	505·38	0·4591	9·2	224·7
(5.) ,, absorption of carbonic acid (dry)	447·58	0·4156	12·3	173·9

Contraction observed.....	62·80	calculated	62·35
Carbonic acid ,, ...	50·80	,,	49·83
Hydrogen in two volumes of gas found	6·076	calculated	6·000
Carbon ,, ,, ,,	2·036	,,	2·000

This gas, treated with an equal volume of chlorine, was exposed to the diffused sun-light, after allowing time for the two gases to mix until nearly colourless, and completed by means of direct sun-light when quite colourless. The bottle was opened under warm water: the hydrochloric acid was absorbed equal to half the capacity of the bottle. The remainder of the gaseous contents not absorbed were displaced by warm water into a receiver, in which a few pieces of stick potash were placed, surrounded by a freezing mixture of salt and ice—a colourless, volatile liquid was condensed.* One hundred grammes of chloride were prepared by the repetition of this process. This first product was separated by distillation into two parts, one which distilled below 30° C., and the other above 30° C. On still further fractionating the first distillate, a portion was obtained boiling at 11—13° C., whose Sp. Gr. was 0·9253 at 0° C. Pierre found the specific gravity of ethyl chloride to be 0·9241 at the same temperature.

The chloride boiling below 30° C. gave, on heating in

* If the gas or the chlorine was not pure, being mixed with air, very little or no liquid was condensed, being carried off by the current. The same was observed by Mr. Schorlemmer. This will probably account for Frankland's observation that no liquid was condensed at —18° C.

sealed tubes with acetate of potash and glacial acetic acid to a temperature of 130°—140° C. for three or four hours, a volatile fragrant liquid, having the characteristic odour of acetic ether, which, after drying over chloride of calcium and magnesia, boiled at 74·0—75·5° C. Kopp gives the boiling point of ethyl acetate at 74·3 under a pressure of 760 mm. of mercury.

In order to prepare the alcohol, the acetate was heated with crystals of baryta hydrate in sealed tubes for one or two hours, to a temperature of 120° C.; after cooling it was distilled, and the distillate treated with dry carbonate of potash until it separated into two layers, the upper one was decanted upon fused carbonate, afterwards upon anhydrous baryta, from which it was distilled, when it had assumed a light amber colour. It began to boil at 78·1° the whole coming over before the temperature exceeded 79·0° C. Kopp gives the boiling point of ethyl alcohol prepared by fermentation at 78·4° C. under a pressure of 760 mm. mercury, and the specific gravity at 0° C. as 0·8095; calculating by means of his coefficient of expansion the Sp. Gr. at 6° C. would be 0·80446, whilst I found the same to be 0·80302 at the same temperature.

The alcohol thus prepared had very little odour, agreeing in this respect with the observation of Meudelejeff *(Zeitschrift Chemie,* 1865, 257*)* though the specific gravity is slightly higher than his at the same temperature calculated from his coefficient of expansion, viz. 0·80123.

On submitting this liquid to combustion analysis the following numbers were obtained.

No. 1. 0·2834 grm. of liquid gave 0·5357 grm. of carbonic acid, and 0·3314 grm. of water.

No. 2. 0·5533 grm. of liquid gave 1·0481 grm. of carbonic acid, and 0·6480 grm. of water.

	Percengate No. 1.	No. 2.	Calculated from the formula C_2H_6O
C	51·56	51·65	52·17
H	12·99	13·02	13·04
O	35·45	35·33	34·79
	100·00	100·00	100·00

The numbers are not all that could be desired when compared with the calculated, this is owing to the difficulty in burning so volatile a liquid, and to the small quantities taken. Dumas and Boullay (*Ann. de Chimic*, 1827, 36, 299) state that in order to obtain agreeing results upwards of a grm. of liquid was necessary, in one combustion 1·742 grms. were used, with ether a still greater quantity was required.

The alcohol still remaining in the carbonate of potash and in the dilute solution was separated by distillation, this distillate was oxidised by a mixture of bichromate of potash and sulphuric acid, when the characteristic odour of aldehyde was recognised, the oxidation was continued until it had disappeared, on distilling to dryness an acid distillate was obtained, this was neutralised with pure carbonate of soda, and yielded on evaporation needle-shaped crystals of acetate of soda; the mother liquor was distilled to dryness with sulphuric acid, the distillate neutralised with pure carbonate of silver, filtered and boiled, on cooling it yielded colourless transparent flat needles, which, after drying over sulphuric acid, gave on analysis the following numbers.

No. 1. 0·4142 grm. of salt gave 0.2663 grm. of metallic silver.

No. 2. 0·5095 grm. of salt gave 0·3274 grm. of metallic silver.

No. 3. 0·3650 grm. of salt gave, after drying at 100° C. in a water bath for one hour, 0·2349 grm. of metallic silver.

No. 4. 0·2483 grm. of salt gave, after drying at 100° C. in a water bath for two days, 0·1604 grm. of metallic silver.

Estimation No. 1 gave 64·30 per cent. of silver.
„ No. 2 „ 64·25 „ „
„ No. 3 „ 64·36 „ „
„ No. 4 „ 64·60 „ „

Calculated from the formula of acetate of silver $\left.\begin{matrix}C_2H_3O\\Ag.\end{matrix}\right\}O$ yields 64·68 per cent. of silver.

That portion of the mixed chlorides which distilled above 30° C. was fractionated, when two-thirds of the total volume distilled over between 57°-59° C. The specific gravity was found to be 1·198 at 6·5° C. Regnault found the Sp. Gr. to be 1·174 at 17° C. and the boiling point to be 64° C. (*Ann. Ch. Phys.* [2] LXXI. 355)* Submitted to analysis the following numbers were obtained.

No. 1. 0·5206 grm. gave 0·3691 grm. of chlorine.
No. 2. 0·4491 „ „ 0·3186 „ „
No. 3. 0·4292 „ „ after drying over stick potash for a week, 0·3010 grm. of chlorine.

Per centage of chlorine found by No. 1, 70·89
„ „ „ No. 2, 70·94
„ „ „ No. 3, 71·76

The percentage required by the formula $\left.\begin{matrix}C_2H_4Cl\\Cl\end{matrix}\right\}$ of mono-chlorinated ethyl chloride is 71·73.

Hence the mono-chlorinated ethyl chloride was formed in quantity by the action of excess of chlorine on di-methyl.

Having commenced the examination of the di-methyl obtained, 1st by Frankland's method from iodide of methyl and zinc, and 2nd by Schutzeuberg's process, I hope to communicate the results to the Society.

* Beilstein (*Ann. Chem. Pharm. V.* 113–110) has shewn that mono-chlorinated ethyl chloride, and chloride of ethyleden, obtained by action on aldehyde with perchloride of phosphorus, are identical, the boiling point of the latter as observed by Wurtz is 58–59, and the specific gravity as determined by Geuther is 1·189 at 4·3° C., these numbers agree with those I found. Beilstein remarks that the higher boiling point as observed by Regnault would result from the presence of higher chlorinated products.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 24th, 1868.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section in the Chair.

The following Gentlemen were elected Officers of the Section for the ensuing year :—

President.
ROBERT WORTHINGTON, F.R.A.S.

Vice-Presidents.
E. W. BINNEY, F.R.S., F.G.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.
MR. THOMAS CARRICK.

Secretary.
G. V. VERNON, F.R.A.S., F.M.S.

"Results of Rain-Gauge and Anemometer Observations made at Eccles, near Manchester, during the year 1867," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The following amounts of rainfall are obtained from two gauges 3 feet from the ground, and one gauge 34 feet from the gronnd. One of the gauges 3 feet from the ground has a round receiver 10in. in diameter, the other has a 5in. square receiver; the edges of both are turned inward. These two gauges stand close to each other on the same plane, 75 feet from my house, and free from every obstruction. The other gauge is a 5in. square one like the one near the ground. It is 4 feet above the ridge of my house, and free from every obstruction. First I represent the rainfall for 1867, as measured by the 10in. gauge 3 feet above the ground and 145 feet above the sea. This I have compared with the average fall for 7 years at Eccles.

Quarterly Periods		1867.	Fall in Inches.	Average of 7 Years.	Differences.	Quarterly Periods	
Average of 7 Years.	1867.					Average of 7 Years.	1867.
Days	Days						
51	55	January	3·306	2·600	+0·706	7·425	7·469
		February	2·590	2·250	+0·340		
		March	1·573	2·575	—1·002		
46	35	April	4·503	1·982	+2·521	7·081	7·641
		May	1·788	2·224	—0·436		
		June	1·350	2·875	—1·525		
50	59	July	5·331	3·310	+2·021	10·526	10·380
		August	1·628	3·289	—1·661		
		September	3·421	3·927	—0·506		
52	51	October	4·100	3·540	+0·560	9·640	10·025
		November	2·159	3·385	—1·226		
		December	3·766	2·715	+1·051		
203	211	Totals	35·515	34·672	+0·843		

This table shows that the rainfall at Eccles has been nearly an inch in excess of the average of 7 years. The summer months from May to September, with the exception of July, had a fall below the average. The wettest months were at the beginning and end of the year. According to the 7 years average April has the minimum, and September the maximum rainfall of the year. The next table shows the monthly amounts that fell in each gauge, and the number of miles of horizontal movement of the air.

1867.	Rainfall in 10in. Round Gauge, 3 feet from Ground.	Rainfall in 5in. Square Gauge, 3 feet from Ground.	Rainfall in 5in. Square Gauge, 34 feet from Ground.	Amount of Horizontal Movement of the Air in Miles.
January	3·306	3·179	2·558	5,396
February	2·590	2·547	1·775	5,334
March	1·573	1·539	1·120	6,884
April	4·503	4·582	4·023	6,867
May	1·788	1·745	1·703	5,137
June	1·350	1·312	1·205	3,817
July	5·331	5·297	4·711	3,677
August	1·628	1·512	1·301	1,744
September	3·421	3·320	2·846	3,389
October	4·100	4·031	3·526	3,371
November	2·159	2·070	1·966	3,018
December	3·766	3·695	3·333	4,121
Totals	35·515	34·829	30·067	52,755

This table shows that the greatest horizontal movement of the air happens in the winter months, or rather from December to April; and as the rainfall appears to diminish

from December to April, it might be expected that the movement of the air and the fall of rain should be the inverse of each other. Yet from the observations of both, which I have, it appears that an excess of windy weather in any month of the year is nearly always attended with an excess of rainfall.

TABLE

Showing the relative proportion of the winds for each month reduced to the four cardinal points, the number of days on which rain fell, together with the average amount received in each gauge.

1867.	Relative Proportion of the Winds to the Days of each Month.				No. of Days on which Rain fell.	Average Amount of Rain received in each Gauge.		
						10in. Round Gauge 3ft. from Ground.	5in. Square Gauge 3ft. from Ground.	5in. Square Gauge 34ft. from Ground.
	N.	E.	S.	W.				
January	8	8	7	8	18	0·183	0·176	0·142
February	3	5	9	11	18	0·143	0·141	0·098
March	9	15	4	3	19	0·082	0·081	0·059
April	6	4	6	14	27	0·166	0·169	0·149
May	7	9	8	7	15	0·119	0·116	0·113
June	12	5	3	10	13	0·103	0·101	0·092
July	12	4	3	12	14	0·380	0·378	0·336
August	9	3	5	14	17	0·095	0·088	0·076
September	10	6	3	11	19	0·180	0·174	0·149
October	7	2	4	18	23	0·178	0·175	0·153
November	10	8	6	6	9	0·239	0·230	0·218
December	11	7	3	10	19	0·198	0·194	0·175
	104	76	61	125	211	0·172	0·168	0·146

TABLE

Showing the average daily fall in each kind of gauge, when the velocity of the wind has ranged between the number of miles indicated in the first column.

Daily Movement of Wind.	No. of Days on which Rain fell.	3 feet from the Ground.		34 feet from Ground.	Difference between (*a*) and (*b*).
		10in. Round Gauge.	5in. Square Gauge. (*a*).	5in. Square Gauge. (*b*).	
Miles.		In.	In.	In.	In.
0 to 50	21	·158	·154	·146	·008
50 to 100	40	·151	·148	·137	·011
100 to 150	32	·178	·173	·154	·019
150 to 200	39	·215	·210	·185	·025
200 to 250	23	·134	·132	·102	·030
250 to 300	28	·208	·205	·169	·036
300 to 350	6	·224	·226	·175	·051
350 to 400	5	·191	·178	·127	·051
Above 400	3	·251	·251	·190	·061
MEAN 200 to 250	22	·190	·186	·154	·032

This table shows that rainy days are most numerous, with a proportionate amount of rainfall, when the horizontal movement of the air, at 34 feet above the ground, is about five miles per hour. But the number of wet days diminishes as the hourly movement of the air increases, though the heaviest falls of rain happen as the movement of the air increases to an average ranging from 8 to 12 miles per hour. The mean difference of the rainfall, 34 feet and 3 feet from the ground, is ·032 inch. This difference is decreased to its minimum in calm weather, and increases to its maximum when the horizontal movement of the air at 34 feet above the ground is about 16 miles per hour.

In May last I commenced to measure the amount of rainfall twice in 24 hours. The results of these measures I represent in the following table.

1867.	Rainfall from 8 a.m. to 8 p.m.	Rainfall from 8 p.m. to 8 a.m.	Difference between the Day and Night Fall.
	In.	In.	
May	0·932	0·856	—0·076
June	0·748	0·602	—0·146
July	3·354	1·977	—1·377
August	0·779	0·849	+0·070
September	2·740	1·681	—1·059
October	2·091	2·009	—0·082
November	1·281	0·878	—0·403
December	1·775	1·991	+0·216

Annual Meeting, April 28th, 1868.

Edward Schunck, Ph.D., F.R.S., &c., President, in the Chair.

Professor John Tyndall, LL.D., F.R.S.; Professor Peter Guthrie Tait, M.A., F.R.S.E.; and Charles Darwin, Esq., M.A., F.R.S., were elected Honorary Members of the Society.

The following Report of the Council was read by one of the Secretaries:—

The Council have again the satisfaction to report that the Society's finances continue in a healthy condition, the general balance of the Treasurer's account on the 1st of April, 1868, being £267 19s. 2d., against £250 2s. 1d. on the 1st of April, 1867.

The number of ordinary members on the roll of the Society on the 1st of April, 1867, was 180, and four new members have since been elected. The losses have been by death, four; by resignations, four; and defaulters, two. The number of members on the roll on the 1st of April, 1868, was 174. The deceased members are, Mr. John Clowes Grundy, Mr. James Aspinall Turner, Mr. Thomas Heelis, F.R.A.S., and Mr. Henry Houldsworth.

Mr. Turner held a high place among the principal merchants and manufacturers of Manchester, and was twice chosen by his fellow citizens (in 1857 and 1859) to repre-

sent them in parliament, which he continued to do till 1865. It may be mentioned that he was the nephew of the Rev. William Turner, who was an honorary member of the Literary and Philosophical Society for many years, and whom he always held in the greatest reverence. While actively engaged in business, and holding several public offices, such as that of Chairman of the Manchester Commercial Association, President of the Natural History Society, and Chairman of Manchester New College, he yet found time to cultivate his strong taste for natural history, and was no mean proficient in certain branches of entomology and botany, and had expended a large amount both of time and money in forming collections illustrative of these subjects.

Mr. Heelis was elected a member of the Society on the 19th of April, 1859, and for three years filled the office of secretary of the Physical and Mathematical Section. He was a very intelligent and accurate observer, and in the course of several voyages during which he visited India, China, the Cape of Good Hope, North America, and the coasts of the Mediterranean, he accumulated a large mass of valuable observations, some of the results of which were given to the Society in the following papers and communications:—

November 10*th*, 1859.—"On Storms, with some attempt to ascertain their tracks in the neighbourhood of the British Islands, and their analogy to other Cosmical Phenomena."

January 5*th*, 1860.—"On R. Bohun's old work 'On the Origin and Nature of Wind,' published at Oxford in 1671."

April 26*th*, 1860.—"On the Observations of the Sun made by Hevelius in the years 1642-3."

November 7*th*, 1861.—Observation of the Zodiacal Light at Smyrna, September 13th, 1861.

March 18*th*, 1862.—"On Abnormal Trade Winds."

November 13*th*, 1862.—"Notes of Observations on η Argûs."

November 13*th*, 1862. — "Hydrometric Observations of the Water of the Mediterranean."

January 27*th*, 1863.—"On Swell observed at Sea, particularly in the Regions of the South-East Trade Wind."

April 2*nd*, 1863.—"Notes of Observations of the Speed and Order of Succession in Magnitude of Waves observed in Gales of Wind off the Cape of Good Hope."

October 15*th*, 1863.—"On Fog in the Mediterranean."

November 12*th*, 1863.—"On a Meteor seen by Daylight."

January 12*th*, 1865. — Account of a Fireball seen December 13th, 1864.

October 31*st*, 1865.—"On a Coal Basin between Mount Olympus and the Bay of Oraniska."

December 7*th*, 1865.—"On Meteors."

December 11*th*, 1866.—"On Cautionary Storm Warnings."

December 26*th*, 1866.—"On some recent Observations on the Specific Gravity of Sea Water."

Mr. Houldsworth was born in Nottinghamshire, on the 4th of June, 1797. His father had much mechanical taste and knowledge, and at the time of his eldest son's birth (the late Henry Houldsworth) he was engaged in developing the spinning mule, and, as afterwards turned out, in laying the foundation of the cotton spinning concern of Thomas Houldsworth and Co., with which his son subsequently became closely connected. The late Henry Houldsworth, though born in Nottinghamshire, was educated chiefly in Glasgow (at which place his father, shortly after the year 1800, went to reside). He was a student in Glasgow University, and about the year 1818 showed his love of science and his powers of invention by publishing in the *Journal of Arts* the first idea of a gas meter. In 1822 he came to live permanently in Manchester, and joined his uncle Thomas Houldsworth in the cotton spinning business. From that year till about 1835, when he became interested in railways, he was continuously engaged

in experimenting upon and improving every process in fine cotton spinning, and in every department he produced improvements, some of which are in use to this day, while others have been superseded. Most of these are only of interest to those engaged in the business with which they are connected. But some, such as "the differential motion" (described to the Society already by Mr. Dyer), various applications to spinning mules, and his improvements upon Heilman's combing machine, have probably some interest in a scientific point of view, as exhibiting the thorough knowledge of mechanical principles which he undoubtedly possessed, as well as an aptitude for invention which was very remarkable, all the more because it was generally applied to what were comparatively trivial subjects. He made very extensive experiments in smoke burning and in raising steam economically. He also improved considerably Heilman's embroidery machine, and during the last few years of his life, when he retired from Manchester, he introduced several important improvements in the construction and adaptation (especially to stiff lands) of the steam plough. This is a general summary of the results of a life extending over 70 years, more marked by constant, conscientious industry and love of knowledge and improvement than by any great achievement.

The following papers and communications have been read at the ordinary and sectional meetings of the Society during the present session: —

October 1*st*, 1867.—"On Sections of Volkmannia," by E. W. Binney, F.R.S., F.G.S.

October 1*st*, 1867.—"On the Portraits of Sir Isaac Newton," by Samuel Crompton, M.D.

October 1*st*, 1867.—"On Pholas holes in the Limestone Rock on Grin Edge, near Buxton," by R. D. Darbishire, B.A., F.G.S.

October 7*th*, 1867.—"On Silk-produciug Worms from Natal," by Mr. A. G. Latham.

October 15*th*, 1867.—"Note on the Occurrence of Sulphocyanide of Ammonium in Gas Mains," by Mr. Peter Hart.

October 15*th*, 1867.—"Jupiter, as observed at Ardwick, on the night of August 21st, 1867," by J. B. Dancer, F.R.A.S.

October 15*th*, 1867.—"Notes on some Superficial Deposits at Great Orme's Head, and the Period of its Elevation," by R. D. Darbishire, B.A., F.G.S.

November 4*th*, 1867.—"Note on the Ship-Barnacle," by Mr. J. Sidebotham.

November 7*th*, 1867.—"Note on the Colour of the Moon during Eclipses," by A. Brothers, F.R.A.S.

November 12*th*, 1867.—"On the Posidonian Schists of the Isle of Man," by E. W. Binney, F.R.S., F.G.S.

November 12*th*, 1867.—"On Exhibitions on the Continent," by R. Angus Smith, Ph.D., F.R.S.

November 26*th*, 1867.—"On a Thermometer unaffected by Radiation," by J. P. Joule, LL.D., F.R.S., &c.

November 26*th*, 1867.—"On Solar Radiation," by J. Baxendell, F.R.A.S.

November 26*th*, 1867.—"Solar Radiation Observations made at Old Trafford, Manchester," by G. V. Vernon, F.R.A.S., F.M.S.

November 26*th*, 1867.—"A Comparison of Solar Radiation on the Grass, and at Six Feet from the Ground," by Thomas Mackereth, F.R.A.S., F.M.S.

December 2*nd*, 1867.—"On Plants appearing in successive years on land prepared for Plantations in Cheshire," by Mr. H. A. Hurst and Mr. George Carter.

December 10*th*, 1867.—"On the Age of the Hæmatite Iron Deposits of Furness," by E. W. Binney, F.R.S., F.G.S.

December 24*th*, 1867. —"Memorandum on the Evaluation of Integrals," by the Hon. Chief Justice Cockle, F.R.S., &c., of Queensland. Communicated by the Rev. Professor Harley, F.R.S.

December 24*th*, 1867.—"On some Points in the Restoration of Euclid's Porisms," by T. T. Wilkinson, F.R.A.S., Corresponding Member of the Society.

December 24*th*, 1867.—"On the Examination of Water for Organic Matter," by R. Angus Smith, Ph.D., F.R.S.

December 31*st*, 1867.—"Solar Radiation Observations made at Eccles, near Manchester," by Thomas Mackereth, F.R.A.S., F.M.S.

January 7*th*, 1868.—"Variable Spot on the Moon's Surface," by W. R. Birt, F.R.A.S. Communicated by J. Baxendell, F.R.A.S.

January 7*th*, 1868.—"On Pholas Borings, as described in Mr. Charles P. Jopling's Sketch of Furness and Cartmel," by E. W. Binney, F.R.S., F.G.S.

January 7*th*, 1868.—"On a Method of determining the Absolute Horizontal Intensity of the Earth's Magnetism," by J. P. Joule, LL.D., F.R.S., &c.

January 7*th*, 1868.—"On the Examination of Water for Organic Matter," Part II., by R. Angus Smith, Ph.D., F.R.S.

January 21*st*, 1868.—"Notes on Some of the Rarer Plants found near Llandudno," by Mr. J. Sidebotham.

January 21*st*, 1868.—"Some Remarks on Crystals containing Fluid," by J. B. Dancer, F.R.A.S.

January 28*th*, 1868.—"On Solar Radiation," Part II., by J. Baxendell, F.R.A.S.

February 4*th*, 1868.—"On some Constituents of Cotton Fibre," by E. Schunck, PhD., F.R.S., &c., President of the Society.

February 24*th*, 1868.—"Remarks on Molecular Activity as shown under the Microscope," by J. B. Dancer, F.R.A.S.

February 25*th*, 1868.—"On the Aurora Borealis as a Weather Prognostic," by Mr. Thomas Ogilvy. Communicated by Murray Gladstone, F.R.A.S.

February 25*th*, 1868.—"On the supposed Influence of the Moon on the Temperature of the Atmosphere near the Surface of the Earth," by J. Baxendell, F.R.A.S.

February 25*th*, 1868.—"On the Rainfall at Old Trafford, Manchester, during the year 1867," by G. V. Vernon, F.R.A.S., F.M.S.

March 3*rd*, 1868.—"On the Lunar Crater Linné," by Herr J. F. J. Schmidt. Communicated by J. B. Dancer, F.R.A.S.

March 3*rd*, 1868.—"Brief Notes on the Laws of Physical Force," by Mr. J. C. Dyer, V.P.

March 17*th*, 1868.—"An Original Letter of the late Sir Walter Scott." Communicated by E. W. Binney, F.R.S., F.G.S.

March 17*th*, 1868.—"Note on an 'Essay on the Resolution of

Algebraic Equations, by the late Judge Hargreave, LL.D., F.R.S.,'" by the Rev. Thomas P. Kirkman, M.A., F.R.S., Hon. Member of the Society.

March 17*th*, 1868.—"On the Origin of Colour and the Theory of Light," Part II., by J. Smith, M.A., Perth Academy.

March 24*th*, 1868.—"Results of Rain-gauge and Anemometer Observations at Eccles, during the year 1867," by Thomas Mackereth, F.R.A.S., F.M.S.

March 31*st*, 1868.—"On the Solution of Algebraic Equations," by the Rev. Thomas P. Kirkman, M.A., F.R.S., Hon. Member of the Society.

March 31*st*, 1868.—"Description of a Dolerite at Gleaston, in Low Furness," by E. W. Binney, F.R.S., F.G.S.

March 31*st*, 1868.—"A Search for Solid Bodies in the Atmosphere," by R. Angus Smith, Ph.D., F.R.S.

March 31*st*, 1868.—"Microscopical Examination of the Solid Particles from the Air of Manchester," by J. B. Dancer, F.R.A.S.

April 14*th*, 1868.—"Brief Notes on the Mutations of Imponderable Elements," by Mr. J. C. Dyer, V.P.

April 14*th*, 1868.—"Researches on Di-Methyl," by Mr. W. H. Darling, Dalton Scholar in the Laboratory of Owens College. Communicated by Professor H. E. Roscoe, Ph.D., F.R.S.

The printing of the third volume, third series, of the Society's Memoirs, containing papers read during the sessions 1864-5, 1865-6, and 1866-7, has been completed; and a new volume has been commenced. Many of the papers in the above list will appear in this volume.

The Sections have again contributed several interesting and important papers and communications to the Society's Memoirs and Proceedings; and as the system of admitting Sectional Associates continues to work satisfactorily, the Council have decided to recommend its renewal for the ensuing year.

The Librarian reports that the Society continues to receive the various publications of the Associations in correspondence with the Society. The number of learned bodies, &c.,

now exchanging their memoirs, &c., with this Society is 272, 196 of which are Foreign, and 76 are British; this is an increase of 16 upon the number at the corresponding period in 1867.

The binding of the works in the Library is making satisfactory progress, 421 volumes having been bound during the last few months, and there are as many more in progress.

The following journals, &c., are regularly purchased by the Society:—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Annals and Magazine of Natural History.
The Quarterly Journal of Science.
The Zoologist.
The Publications of the Palæontographical Society.
" " " Ray Society.
" " " Cavendish Society.
The Quarterly Journal of Pure and Applied Mathematics.
Genera plantarum, par G. Bentham et J. D. Hooker.
Les Mondes: revue hebdomadaire des sciences, par M. l'Abbé Moigno.
Journal de l'Ecole Impériale Polytechnique.
Les Annales de Chimie et de Physique.
Les Annales des Sciences Naturelles; Zoologie et Botanique.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Prodromus systematis universalis regni vegetabilis, par Alphonso de Candolle.
Die Astronomische Nachrichten.
Die Annalen der Physik und Chemie.
Die Annalen der Chemie und Pharmacie.
Die Journal fur die reigne und angewandte Mathematik.
Neues Jahrbuch fur Mineralogie, Geologie, und Palæontologie.
Die Zeitschrift fur wissenschaftliche Zoologie.
Allgemeine Encyklopädie der Physik.
Astronomische Beobachtungen zu Bonn.

On the motion of Mr. W. B. JOHNSON, seconded by Mr. H. WYLDE, the Annual Report was unanimously adopted.

On the motion of Mr. BROTHERS, seconded by Mr. BINNEY, it was resolved unanimously —"That the system of electing Sectional Associates be continued during the ensuing session."

The following gentlemen were elected officers of the Society for the ensuing year: —

President.

JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S., &c.

Vice-Presidents.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
JOSEPH CHESBOROUGH DYER.

Secretaries.

HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S.
JOSEPH BAXENDELL, F.R.A.S.

Treasurer.

ROBERT WORTHINGTON, F.R.A.S.

Librarian.

CHARLES BAILEY.

Of the Council.

REV. WILLIAM GASKELL, M.A.
PETER SPENCE, F.S.A., F.C.S.
GEORGE VENABLES VERNON, F.R.A.S.. F.M.S.
JOHN BENJAMIN DANCER, F.R.A.S.
WILLIAM JACK, M.A.
WILLIAM LEESON DICKINSON.

R. WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

Dr. — From 31st March, 1867, to 31st March, 1868. — Cr.

1867.	£ s. d.	£ s. d.
April 1.—To Balance in Bank of Heywood Brothers and Co.	314 9 1	
,, Balance in Treasurer's hands	8 3 0	
		322 12 1
1868.		
Mar. 31. To One Composition		26 5 0
To *Members' Contributions:*		
Arrears, 1865-66 ..1 ..	2 2 0	
,, 1866-67 ..12 ..	25 4 0	
Members on the Roll, April 1, 1867, 180 at 42s. ..£378 0 0		
Deduct Compounders ..4		
,, Members deceased..2		
,, Members in Arrear, March 31, 1868 ..14 ..42 0 0		
20	336 0 0	
Four Members elected in 1867, April—December	8 8 0	
Four Admission Fees	8 8 0	
Seven Associates of Microscopical Section at 10s.	3 10 0	
One Associate Arrears ditto at 10s.	0 10 0	
Five Associates of Photographic Section at 10s. 6d.	2 12 6	
One Associate of Physical and Mathematical Section at 10s. 6d.	0 10 6	
		387 5 0
,, To *Sale of Publications:*		
Proceedings		1 3 0
,, To *Sundry Income:*		
Sec. Contrib.: Microscopical..£2 2 0		
,, ,, Photographic.. 2 2 0		
,, ,, Physical and Mathematical .. 2 2 0		
	6 6 0	
Interest allowed by Bankers	4 6 5	
		10 12 5
		£747 17 6
Composition Fund ..£98 15 0		
General Balance .. 267 19 2		
	£366 14 2	

March 31, 1868.	£ s. d.	£ s. d.
By *Charges on Property:*		
Chief Rent	12 14 0	
Fire Insurance	5 12 6	
Inhabited House Duty	6 7 6	
		24 14 0
,, *House Expenditure:*		
Water, Gas, Candles, and Coal	18 9 7	
Cleaning and Petty Expenses	2 4 11	
Furniture	1 14 4	
Repairs	4 11 1	
Tea and Coffee at Meetings	18 17 2	
		45 17 1
,, *Administrative Charges:*		
Wages—Keeper of Rooms	52 0 0	
Attendance on Sections	6 6 0	
Postage and Parcels	20 16 4	
Stationery, Printing Circulars, &c., and Receipt Stamps	16 6 5	
		95 8 9
,, *Publishing:*		
Memoirs—Printing, Engraving, &c.	50 5 6	
Printing Proceedings	56 9 0	
Editor of Memoirs and Proceedings	50 0 0	
		156 14 6
,, *Library:*		
Periodicals, Binding Books, &c.	55 6 0	
Subscription to Palæontographical Society	1 1 0	
,, to Ray Society	1 1 0	
,, to Cavendish Society	1 1 0	
		58 9 0
Total Disbursements		381 3 4
Balance in Bank of Heywood Brothers and Co.		366 14 2
		£747 17 6

17th April, 1868.

ROBERT WORTHINGTON, Treasurer.

Audited and found correct, April 20, 1868.

WM. BROCKBANK, ALF. BROTHERS, } Auditors.

The following letter from Sir John F. W. Herschel, Bart., M.A., F.R.S., &c., dated Collingwood, April 28th, 1868, was read :—

I am so accustomed to see passages from my own writings cited in direct opposition to their literal meaning and in support of views directly adverse to those entertained by myself, and which they were written to explain or to advocate, that I should hardly have thought it worth while to notice one more instance of the kind in the paper read at the meeting of the Phil. and Lit. Society on the 14th inst. by Mr. J. C. Dyer, but for a general impression of the inexpediency of silence on such occasions. I allude to his statement that in treating of the undulations of light I have somewhere or other estimated and given in figures the amount of "resistance to the transit of light through space" of what he terms the "luminous ether," (the double commas in this last case being his own)—meaning, I suppose, the luminiferous ether—a distinction ever by myself most carefully attended to. This is very much as if one should speak of the resistance of air to the propagation of sound—of steam power to the conveyance of information by mail-train or of electricity to its conveyance by telegraph—or of the resistance of gunpowder to the propulsion of a cannon ball. It is true that in the article cited I have endeavoured to estimate and to state in figures the intensity of the elastic forces *effective in the propagation* of light—but assuredly I never expected to see this construed (if that be the estimate referred to) into the exact opposite of what it obviously means.

If, on the other hand, Mr. Dyer should have had in view any passage in any other of my writings in which I may have spoken of the doctrine of the imperfect transparency of the celestial spaces, propounded (if I mistake not) by Olbers; I may be allowed to explain once for all, that I never advocated that doctrine and do not believe in it.

Mr. DANCER exhibited some interesting specimens of American photography, lent by Thomas Higgins, Esq., of Liverpool. One of these pictures represented a portion of the valve of the Diatom Pleurasigma angulatum. This object has on its surface striæ estimated at 52,000 in the inch. This was magnified 2,344 diameters by one of Powell and Lealand's objectives of 1-50th of an inch. Another of the same object was magnified 2,540 diameters with one of Wale's 1-8th objective with amplifier. Another picture exhibited these photographs enlarged to 19,050 diameters. Notwithstanding the immense magnifying power employed, these photographs were remarkably sharp and distinct. Some of the other photographs exhibited magnified portions of the Podura scale. These photographs were taken by Brevet-Major Curtis, of the U.S.A.

"Notes on some of the Characteristic Plants of the Channel Islands," by CHARLES BAILEY, Esq.

Two visits to the Channel Islands, made in July, 1863, and August, 1867, enable me to record additional localities for some of the species, as well as exhibit others which have not hitherto been included in the flora of the Islands.

Ranunculus peltatus, Fries. var. *a*, *vulgaris*, E. B. A dwarf form of this plant occurs on L'Ancrene Common, Guernsey, and in marshy ground at the eastern end of St. Brelade's Bay, Jersey.

R. Flammula, L. var. β, *pseudo-reptans*, E. B. St. Saviour's Valley, Jersey.

R. ophioglonifolius, Vill. I exhibit the foliage only, for though this species is very abundant in St. Peter's Marsh, Jersey, the peculiar tenure on which the marsh is held causes it to be mown as smooth as a bowling green early in the year.

Matthiola sinuata, R. Br. A few seedlings from St. Brelade's Bay, Jersey; but the fine specimens in the room, showing the muricated glandular pods, were gathered in the neighbourhood of St. Owen's Pond, where the plant is abundant.

Branica adprena, Bois. Plentiful on the western shore of Jersey.

B. tenuifolia, Bois. Common in Jersey, though not included in the lists of plants of that island.

B. Cheiranthus, Vill. Met with only on the embankment of the new road between St. Aubyn's and St. Brelade's.

Helianthemum guttatum, Mill. Forming almost the entire herbage of some parts of the sandy tract of St. Brelade's Bay; it is common also at l'Etac. All the specimens are much branched, some of the plants making little bushes a foot or more in height. Compared with *H. Breweri*, Planch, the flowers are smaller and of a much paler yellow than in the Anglesea specimens shown by Mr. Sidebotham at a recent meeting. The spot at the base of the petals is of a livid brown, and very different from the larger bright reddish-chocolate of that plant; the seeds are very similar, the pearly tubercles being perhaps closer and better developed in the Jersey plant.

Dianthus prolifer, L. Plentiful in the lanes leading from St. Owen's Manor House to the shore; also at L'Etac Point, Jersey.

Silene conica, L. Plentiful on the Quenvais, Jersey.

Cerastium quaternellum, Fenzl. Not infrequent in Herm.

Spergularia rupestris, Lebél (non Camb.). The commonest species of Spergularia in the islands, though often passed over as *S. marina*. Its longer peduncles, black seeds, and glandular pubescence are ready marks of identification. Though generally found on walls and rocks, some of the specimens shown were gathered in salt water ditches.

Lavatera arborea, L. Cobo Bay, Guernsey.

Hypericum linariifolium, Vill. A few plants met with in a glen near St. Aubyn's, Jersey. Compared with continental examples the Jersey plant is much more stunted, with more of the habit of *H. humifusum*.

H. Elodes, L. Plentiful in a marsh at St. Brelade's, Jersey.

Oxalis corniculata, L. A common garden weed in the north of Guernsey; also near St. Peter's, Jersey.

Ulex Galii, Planchon. This species occurs near the martello tower, St. Brelade's, Jersey. No doubt it is often passed over as *U. nanus*.

Trifolium enincarnatum, E. B. Common in Jersey, where it is cultivated for fodder.

Lotus diffusus, Tm., and *L. hispidus*, Desf. Common in Guernsey, and especially so on the headland which terminates in Jerbourg Point. I am unable to separate these species with satisfaction, the length of the pod, direction of the beak, and number of the flowers varying considerably in the Channel Island specimens.

Rubus thyrssidens, E. B. 3rd. ed. Above St. Peter's Port, Guernsey.

Epilobium lanceolatum, Sebast et Maur ? I quote the name of this species with doubt for a plant occurring on road sides in southern Guernsey.

Œnothera odorata, Jacq. Well-established on the sands of St. Brelade's Bay, Jersey. Near the same locality grows Exholtzia californica, and a species of Mesembry anthemum.

Polycarpon tetraphyllum, L. In considerable abundance in the eastern and southern parts of Guernsey. It is much less plentiful in the other islands.

Bupleurum aristatum, Bartl. L'Ancrene Common, Guernsey.

Galium elatum, Thuill. Plentiful near St. Peter Port, Guernsey.

Dipsacus sylvestris, L. Near St. Owen's Church, Jersey.

Hieracium strictum, Fries. I collected on the rocks in the northern part of Sark some plants which I am inclined to refer to this species.

H. umbellatum, Vill. Narrow-leaved, slender plants of this species are plentiful on Noirmont Point, Jersey.

Tragopogon parrifolius, L. Common in Jersey.

Diotis maritima, Cass. In great luxuriance near the Martello Towers, St. Owen's Bay, Jersey.

Gnaphalium luteo-album, L. The Quenvais, Jersey, but infrequent.

Petasites fragrans, Presl. In a small copse near St. Aubyn's, Jersey, but apparently not flowering.

Imila crithmoides, L. Plentiful on the rocks near the the silver mine, Sark; and in salt water ditches near St. Sampson's, Guernsey.

Cuscuta epithymum, L. Covering the Gorse everywhere; it is called by the islanders "Fairies' Hair."

Scrophularia Scorodonia, L. In great abundance and luxuriance in the fosse of Fort George, Guernsey.

Linaria arenaria, D. C. Occurs in great plenty on the neighbouring shores of Normandy and Brittany, and it is not unlikely to be found in Jersey.

Orobanche Hederæ, Duby. Common in Guernsey.

Mentha rotundifolia, L. Abundant by the side of a brook running into Moulin Huet Bay, Guernsey.

Calamintha Nepeta, Clairv. In the greatest profusion in Herm, particularly on the eastern side.

Leonurus Cardiaca, L. La Havre Gosselin, Sark.

Scutellaria minor, L. Common in a marsh over Bonley Bay, Jersey.

Echium plantagineum, L. South-western parts of Jersey; frequent.

Armeria plantaginea, Willd. Covering the sands in the western and southern parts of Jersey; exhibiting great variety in the breadth and vernation of the leaves.

Atriplex arenaria, Woods. Plentiful St. Brelade's Bay, Jersey.

Orchis laxiflora, Lam. Marshy meadows to the south of St. Owen's Pond, Jersey, with Spiranthes autumnalis.

Allium sphærocephalum, L. A few plants met with amongst old timber in St. Aubyn's Bay.

Scilla autumnalis, L. Very luxuriant at Noirmont Point, and Point La Frette, Jersey. The absence of bracts is often relied upon as a specific character, but I noticed many examples in Sark bearing these appendages.

Alisma ranunculoides, L. var β, *repens*, Sm. L'Ancrene Common, Guernsey, and St. Brelade's, Jersey.

Ruppia maritima, L. In salt-pans near the Vale Church, Guernsey.

Cyperus longus, L. More plentiful in Guernsey than in the other islands. The farmers leave patches of this plant unmown in their meadows, in order that it may be gathered in the autumn for making into cordage; the fishermen consider that ropes made from this plant wear much better in the sea than ordinary ropes.

Cladium Mariscus, R, Br. St. Owen's Pond, Jersey, growing with Scirpus Tabernœmontani and Scirpus pungens. It is remarkable that so conspicuous a plant has not hitherto been recorded as occurring in the islands.

Polypogon monspeliensis, Desf. Road-sides, St. Sampson's, Guernsey.

Lagurus ovatus, L. In the greatest profusion on the shores of L'Ancrene Bay, and Grande Havre, Guernsey.

"Brief Notes on Latent Heat," by J. C. Dyer, Esq., V.P.

The origin and nature of Heat having engaged so large a share of scientific attention of late years, and directly

conflicting principles in physics having been assumed, to explain calorific phenomena, render it desirable to obtain, if we can, some reliable tests of the several grounds on which the opposing theories are based.—On the one side it is affirmed that, "Heat is a material element pervading the universe." On the other, "that it is, in fact, a nonentity, and merely the result, or consequent effect, of the exertion or expenditure of moving forces."—In purely physical enquiries we must be guided by ascertained facts, rather than by authority, even of the most eminent physicists; wherefore I shall adduce some of the well-known facts bearing upon the points at issue. The materiality of heat cannot be sustained, except upon the admission of Dr. Black's doctrine of latent or specific heat in bodies; on this assumption, then, I shall cite some of the cases that go to prove the existence of heat in bodies, beyond what is sensible or thermometric in them; and, therefore, in a neutral or latent state. And first, I take the results of the carefully conducted experiments of Dr. Fairbairn, showing the relations of heat and water in their different forms of union. The following passage is from his valuable work:—"Useful information for Engineers," London, Longman & Co., 1866. pp. 3–4. "When water attains the temperature of 212° Fahr. under the atmospheric pressure, it boils and gives off steam, which, when slowly formed, contains about 934·6° of latent heat, or heat not indicated by the thermometer; if to this be added the sensible heat of 212°, we have 1146·6° of heat required to raise, say one pound of water into steam, under the atmosphere of 14·7lbs. pressure. But at a pressure of 25lbs. per inch, the boiling point rises to 240°, at 50lbs. to 281°, and at 100lbs. it is of 327½° of temperature." The temperature or sensible heat in steam is thus gradually raised by the increasing pressure or density of the steam,

and saturated steam, or steam raised in a boiler over water, is found to take up nearly double quantity of water to form steam of double density or pressure; but the latent heat in steam is directly as the quantity of water converted into steam, whilst the sensible heat in it is only as above stated, viz. :—

from 212° at 14·7lbs.,	to 240°	at	25lbs.,	increase	28°
„ 240° at 25lbs.,	to 281°	at	50lbs.,	„	41°
„ 281° at 50lbs.,	to 327½°	at	100lbs.,	„	46½°

The decreasing latent heat in steam is found to be nearly as the increasing sensible heat, or, in other words, the absolute heat is nearly in proportion to the water changing its form of a liquid to an elastic vapour.

By the increase of sensible heat, all liquids and solids are gradually expanded, and by the abstraction of such heat, they are contracted in bulk; but the degrees of expansion and contraction are different in bodies, as shown in the mercurial and spirit thermometers, the Wedgwood pyrometers, and the different metallic rods used for compensation balances and pendulums. In all such cases, the gradual changes of bulk by the gain and loss of sensible heat, the *law* governing the changes is quite distinct from that which rules in the cases of sudden changes of water and other liquids into steam and again back into the liquid form, and by which changes of constituent forms, absorption and liberation of heat takes place, to the extent before shown, beyond what is indicated by the thermometer.

It thus appears quite clear that the entering of heat latent in steam is a consequence of water being converted from its liquid state to that of aqueous vapour, and the reappearance of the same heat, in a sensible state, when the

steam is converted into water, is equally the obvious consequence of the change. Such reciprocal changes of the constituent forms of bodies, clearly serve to explain the changes of heat into its latent and sensible forms, as before stated. We find too, all solids show the *gradual* loss and gain of heat, however high the temperature may be raised, unless the body itself be converted into a liquid or vapour, and then the sudden absorption of latent heat in them is found, as in the case of steam from water. Taking the total amount of sensible heat in steam of 50lbs. pressure (as in common use) at 281°, as above shown, and the latent heat in the same steam, as given in the tables, at 2,670°, the total absolute heat in such steam will equal 2,951 degrees Fahr. Now, if the entire heat so contained in steam of 50lbs. pressure, were to remain thermometric heat, it would of course raise the temperature of the pipes and engine to the same degree, which would soften the metal and destroy the vessels conducting it, to say nothing of the danger of attending or even approaching the engine. I am aware of the allegation, that heat, not appearing sensible in steam beyond the boiling point, is "because of the expansion of water into some 1,640 times its liquid bulk when converted into steam." Well, even though we allow this way of accounting for the *insensible* state of the heat in steam, still the fact remains that the 934·6° of heat exists in the steam, for that amount is carried over by it to the condensor, and reappears as sensible heat in the water.

From these facts it would seem that the *term* "latent heat" is the only matter in question; for the entire amount of heat, say of 934°, that disappears as sensible heat in the steam, reappears sensible in the condensor. It is quite

familiar to all, that by the forming of steam, as in common evaporation, sensible heat is absorbed, and the air cooled, also that when aqueous vapour is condensed, heat is given out; so that heat is thus continually changing its form from sensible to latent, and *vice versâ*, by the changes of water into the alternate states of liquid and vapour.

The same phenomena, but to a less extent, appears in the freezing and thawing of water, namely, when a pound of ice is melted in warm water, the amount of sensible heat required to convert it into water is about 140° Fahr. more than appears in the water, viz,, the heat to maintain its liquid form is 140° in excess of what is contained in the state of ice at the melting point; and when the same pound of water is again converted into ice it gives out the said 140° of latent heat from the liquid in the act of freezing.

A hundred other cases equally familiar might be adduced to show the disappearance and reappearance of sensible heat in bodies upon their changes of constituent forms, as solid, liquids, and vapours, and in all such cases the increments and decrements of heat, in its sensible and latent states, take place suddenly, just as the bodies undergo those changes, and in strict accordance with known scales of specific heat in them, as published in the tables of such heat.

I may here glance at another aspect of latent heat. We have seen that steam of double density, viz., from 25lb. to 50lb. per inch, is raised by the addition of 41° of sensible heat; and again, from 50lb. to 100lb. pressure by the further absorption of 46½° of sensible heat. These small accessions of heat in its sensible state are accounted for by the amount of latent heat in the extra water, as before stated; for, to

double the pressure of isolated steam requires some 460° of sensible heat.

If we look to the 140° of latent heat in water over that in ice, it will be seen that in lieu of being expended by the heat thus absorbed in melting the ice, the water is contracted in bulk; for at temperatures from 32° to 40° ice floats with about one sixth of its bulk above the surface. The notion, therefore, that heat ceases to be sensible by reason of the expansion of bodies when it becomes latent in them is clearly shown to be a fallacy.

I come now to notice the latent heat that becomes sensible by the union of a solid with a gas. The latent heat in oxygen and carbon being in excess of that in carbonic acid, by the formation of this acid in breathing, the supply of sensible heat is duly maintained to keep up the temperature of warm-blooded animals, and the excess of heat evolved in the lungs is carried off by converting water into steam, which passes out with the breath. If the whole heat so evolved were to remain sensible, it would destroy the living functions.

Many other mutations of heat depending on the vital energies were explained in a former paper, and may be here passed by, with the remark that this entire class of changes, as in the case of respiration, are effected by chemical forces called into action by vital organisms. The alternate changes of sensible and latent heat depending on the constituent forms of bodies, and upon the chemical forces so called into action, might be shown in a hundred other cases, but the above will suffice to mark the principle on which the mutations of heat depend. Apart then from the chemical relations of heat

with other bodies, I proceed to notice some of the cases in which the appearance and disappearance of sensible heat that do depend on the action of mechanical forces; these are of so wide a range, that only a few of them can or need be given. It must be kept in view that the latent heat in bodies, besides depending on their constituent forms, as solids, liquids, vapours, and gases,—is also proportionate to their bulks, in the same form; wherefore, by compressing a body of any kind, sensible heat is given out, and by expanding it sensible heat is re-absorbed, or taken up in a latent state; so that mechanical forces continually acting among bodies around us bring about the alternate conditions of heat so familiar to everyone. The cause therefore is thus plainly seen why sensible heat in bodies is produced by mechanical forces. But this affords no ground for assuming the generation *de novo* of any heat by the exertion of any force—either chemical or mechanical.

We have sensible heat from the solar rays, and that produced by combustion, and other chemical changes of bodies, but have strong grounds for concluding that all forms of sensible heat produced by mechanical forces are derived from the latent heat, rendered sensible by compression or the abrasion of bodies by such forces. But the alternation of sensible and latent heat thus caused by mechanical action is seen in an endless range of compressions and expansions from the lighting of a match in the air syringe to the showers of sparks issuing from the particles of steel torn off in *dry grinding,* and ignited by the intense heat from the compressed air between the steel blade and the grindstone. I

need not enlarge on the numerous other facts in accordance with the foregoing, but may venture to say that those cited must be directly met by adducing others to show *how* sensible heat is made to come and go otherwise than by the changes of heat from one to the other of its states of sensible and latent, before the principle of such mutations of heat can be superseded by the new theory. To say that "*work is done,*" when water passes into steam, equal to the heat entering the steam,—and, again, that when the steam is condensed mechanical force is excited equal to the heat re-appearing in the water,—are naked assertions, with no shadow of proof to sustain them, for no mechanical force is called into action in forming the steam, nor is any such force exerted by it in returning to water. So that *force* has nothing to do with those changes, except it be the chemical forces, depending on the known relations of heat and water. We all know that there is nothing *occult* in the laws of dynamics, and that all moving forces can be distinctly *measured* and their amount and direction plainly proved, which forbids the admission of any mystery in the science of mechanics. Now, in the face of these established laws, we find the following announcement by the able expounders before cited of the New Philosophy, viz.—"When the motion of a body is arrested, the motion of the mass is transferred to a motion of the molecules—or particles composing it—and this motion is heat." And again, that "Mathematical formulæ have been deduced from it, and thus a quantity has been found, termed the Mechanical equivalent of heat." That the motion of the mass is transferred to the molecules is an assumption devoid of any foundation in known facts, for we have no means of proving any kind of dynamical action

by such intangible, unknown, and suppositious molecules, or ultimate particles of bodies. And to base mathematical formulæ on such visionary forces and motions can hardly comport with the dignity of the exact sciences, or tend to shed any light upon physical truths, which after all must be sustained by known facts, rather than by theories, old or new. As these two theories are based on opposite principles, they cannot both be true; and without referring here to any of the other cases (adduced in former papers) that appear to prove the untenable nature of the new theory, I conclude by submitting the above short outlines of the phenomena resting on, and explaining, the doctrine of latent heat.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 23rd, 1868.

J. B. Dancer, F.R.A.S., President of the Section, in the Chair.

Mr. Sidebotham stated that on opening a box containing specimens of Limnœa clodes from Canada, sent to him by Prof. Billings, a living bee, apparently the common hive-bee, crawled out.

Dr. Alcock showed a living frog-tadpole, two and a half inches long, with a body as large as a hazel nut, but the hind legs not more than a quarter of an inch long. He stated that it was taken from a pond in Higher Broughton on the 7th of March, at a time when the frogs were beginning to spawn. It was evident that it had lived through the winter, growing larger but advancing very little in development; and he could only account for its occurrence in that condition by supposing it had grown from spawn deposited in the autumn of last year.

Mr. Sidebotham read the following paper, entitled "Further Notes on Wood-feeding Coleoptera."

A short time ago I brought before you a few notes on some of the wood-feeding beetles found in this neighbourhood, and pointed out the very great damage they cause to growing timber. I will now refer to a few of the species which chiefly attack timber when it has been cut down and used for various purposes.

It appears to be one of the provisions of nature that as speedily as a tree is separated from its root it is destroyed as soon as possible, and various means are at once brought into play to this end. Very soon after the tree falls, it is attacked by various species of coleoptera, the bark mined and the trunk pierced in all directions, the sharp mandibles of the little creatures working into the hardest wood. The holes thus made let in the rain, and then the half-rotting wood is attacked by other species, and in a very short time the whole is reduced to dust, which if left to its own natural decay would encumber the ground for years.

Unfortunately, these little beetles cannot distinguish between what is of value and what is not; stumps and rails look to them the same as old trees, and they treat them in the same way.

In this part of the country, three species of the genus Anobium, and Ptilinus pectinicornis, another nearly allied species, are the most destructive. We often see rails and gate posts perforated as if by small shot, and if we examine them carefully we find the inside of the timber a mass of dust.

Where timber is of much value, or where much labour is spent upon it, some steps ought to be taken to prevent its destruction, either by kyanizing it, or by coating it with paint and varnish before it is attacked. Anobium tesselatum, and Anobium striatum, are species that feed upon wood and furniture, in the insides of buildings, and often cause great destruction before their presence is known, although the first named, which is the well-known death watch, makes itself heard, often to the alarm of the ignorant; but as a writer 150 years ago remarks, "We look on it as an idle superstition, for were anything in it, inhabitants of old houses were in a melancholy condition." It appears to be an instinct of these beetles to remain inside the timber, burrowing their holes in all directions, and when they come

near the surface to turn away, without making a visible hole. The female rarely leaves the burrows, but lays her eggs inside them, the only holes apparent being those by which the males have escaped. Thus it may happen that a beam of a house apparently strong, and in which only a few small holes are to be seen, is in reality a mere shell full of dust. My cousin, the late Mr. Ashworth, once shewed me a large box full of wooden silk bobbins. They were of a size not often wanted, and had remained in this box about two years; when required, it was found the whole of the wood had been eaten by Anobium striatum, leaving the exterior of the bobbins of the thickness of paper, and apparently quite perfect.

No wood appears too hard or too dry for these creatures. A piece of furniture which had been in my possession many years, and had belonged to my father, was attacked by this species. The first intimation I had was the finding of some of the beetles inside the window of my bedroom, and on searching I found the holes from which they had escaped. I at once had the wood washed with a solution of bichloride of mercury, and the holes well saturated with it, and have since seen no more.

In a private house the presence of this beetle may be soon seen, but in a church or public building much injury may be done before it is discerned. When it is, the timber infected should, if possible, be removed, or well washed with bichloride of mercury and mineral naptha. It should also be covered with a fine net in the month of May, so that the beetles then assuming a perfect state and emerging from the timber could be prevented going to other parts of the building.

The injury to timber is caused almost entirely by the larvæ, the perfect insects simply drilling holes to make their escape on their arrival at maturity. Remembering this, and knowing the habits of the various species, and the

times when they leave their holes, much damage may be prevented by the destruction of the perfect insects before they have time to lay eggs. A year or two ago, a friend called my attention to a small grub, which was eating the corks of his wine bottles, and doing considerable damage by boring holes through the corks and letting out the wine. The grub was the larva of a small beetle, Mycetæa hirta, and I reared some of the beetles from the larvæ in one or two corks. The perfect insects emerge early in June, and I think if the wine cellar in such a case were well white-washed at that time, when the insects are on the ceiling and walls, they would be destroyed; and if the corks were covered well with sealing wax, one or two seasons would be sufficient to banish this beetle altogether from the place.

I may just remark that I think it would be quite worth the trouble of every one to know some little of the appearance and habits of the beetles usually found in houses, if for no other reason than to preserve their property. A wasp destroyed in the spring is the destruction of what would be a wasps' nest in the autumn; in like manner, the capture of a small beetle on a window may prevent the destruction of a valuable cabinet, or the injury of an entire library.

April 20th, 1868.

J. Baxendell, F.R.A.S., in the Chair.

Mr. Charles Bailey read a paper "On Some of the Characteristic Plants of the Channel Islands," and exhibited a large collection of specimens.

[This paper was afterwards read at the annual meeting of the Society, held on the 28th of April, 1868. See p. 194.]

The SECRETARY read a communication from the Rev. J. E. Vize, "On the Mite of the Wood Pigeon."

It was observed that within a short time of the death of the pigeon these mites appear much more frequently than when the body of the bird was warm; that they come and go very much like a dissolving view; moreover, they always make ingress and egress through the vanes of the feathers of the bird; these they open, and then pass away, not head first, but just as a heavy weight would do when left on water to gravitate for itself.

They are very nimble, and always clasped the feather by at least two of their feet, never less. Hence their tenacity, for it is most difficult to remove them from their holdfast. And here is a beautiful and natural means of self-preservation: if they lost their security and became detached from the bird in flight, they could not regain their place, and probably must die. They walk differently to most insects, using invariably the central foot on the right side, and the first and last of the opposite side, and *vice versa*. When quiescent they adhere very tightly by the pair of feet nearest to the head, being aided in this by a pair of very small, but most useful, clips or hooks at the extremity of each of their feet.

As to the antennæ, they denote very great excitement being in rapid restless movement. Whilst the insect is alive, they are at something like right angles to the body of the bird, but are nearly always parallel to the body when dead.

Annual Meeting, May 4th, 1868.

A. G. LATHAM, Esq., Vice-President of the Section, in the Chair.

The Annual Report was read, approved, and ordered to be printed and circulated among the members.

The following Officers were elected for the ensuing session :—

President.
JOHN B. DANCER, F.R.A.S.

Vice-Presidents.
ARTHUR G. LATHAM.
JOSEPH BAXENDELL, F.R.A.S.
JOHN WATSON.

Secretaries.
THOMAS ALCOCK, M.D.
CHARLES BAILEY.

Treasurer.
HENRY ALEXANDER HURST.

Of the Council.
W. C. WILLIAMSON, F.R.S.
ALFRED BROTHERS, F.R.A.S.
SAMUEL COTTAM.
THOMAS COWARD.
JOSEPH SIDEBOTHAM.
JAMES G. LYNDE, F.G.S., F.R.M.S., M.Inst.C.E.
HENRY SIMPSON, M.D.
JOHN BARROW.

Mr. C. Bailey exhibited a sample of the shell-beach at Herm, and explained how shells are thrown up at that particular spot in consequence of the direction of the several currents around and between the neighbouring islands.

Mounted specimens of Meridion circulare from the entrance to the Peak Cavern, Castleton, were sent for exhibition by Mr. Dancer, together with a stock of the same Diatoms presented for distribution.

Leaves of Hippophaë rhamnoides from Folkstone were presented by Mr. Sidebotham for distribution among the members.

PHYSICAL AND MATHEMATICAL SECTION.

April 21st, 1868.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

"On Ozone and its Probable Connexion with Solar Radiation," by THOMAS MACKERETH, F.R.A.S., F.M.S.

Many hypotheses have been set forth respecting this mysterious something, ozone. My present object is to bring together as many known facts as I am able respecting it, and to draw therefrom such conclusions as I think fairly legitimate. The deductions that may be drawn from experiments made in a chemical laboratory are no doubt important and useful; but if to these we can add observations from the great laboratory of nature, we may hope to arrive at fuller and better information.

First.—*Where ozone cannot be found.* Attempts were made for some time to detect it in the vicinity of the Manchester Royal Infirmary, but without success. Mr. Samuel Marshall, of Kendal, states that "in a locality west of Birmingham, they have indications of the presence of ozone when the wind is from the west, or before it reaches the town; but when the wind is in the opposite quarter, and passes over the town before reaching there, none is found." In addition to this evidence of the absence of ozone in towns, I can state that, from observations I have made during the last six months in Salford, hardly any has been detected with the most sensitive tests. Hence it is clear that large towns either destroy ozone or prevent its manifestation.

Second.—*Where it is partially found amongst populations.* I lived for four years in close proximity to the most populous part of the village of Eccles. This village contains a population of two or three thousand. Its sanitary condition is most unsatisfactory. Its mean height above the sea is about 112 feet. It is situated about four and a half miles west of Manchester. I made observations of ozone at that station for more than three years, and the amounts detected with Dr. Moffat's test papers were considerably below those obtained by Mr. Vernon, F.R.A.S., at Old Trafford, two miles nearer Manchester, but where the population is less dense and the sanitary condition far superior. I infer, then, that density of population, with imperfect sanitary regulations, either destroys ozone or prevents its formation.

Third.—*Where variable amounts of ozone are found out of large towns.* Ozone is detected in considerable quantities on the sea-coast, and in rural valleys far removed from towns; but it is well known that far larger amounts are detected in elevated districts, as on hills and mountains. The amounts detected by Mr. Heap, of Royton, near Oldham, and Mr. Thompson, of Wilmslow, far exceed anything that is found on low rural lands or near the sea. Mr. Heap's station is 544 feet above the sea, and Mr. Thompson's 251 feet. Mr. Samuel Marshall, of Kendal, also writes—"I am much surprised at the results of experiments made by my friend Edward Hayton, at his residence, High House, which is about 7½ miles N.W. from Kendal, and two miles nearly west from the railway station at Windermere—636 feet above the sea level. * * * No material interruption of hills between it and the sea, Morecambe Bay, which lies a little west of south. The amount of ozone collected here is surprising, being nearly three times the quantity of that at Kendal. It may be said that sea breezes conveying ozone from the Irish Sea meet with no material obstacle before

reaching High House; but they meet with comparatively little before reaching Kendal, and yet the results are so very different." Mr. Marshall's hypothesis that ozone comes from the sea does not enable him to discover why there is more ozone at High House than at Kendal, which is several miles nearer the sea. During my several visits to Westmoreland I have made ozone experiments, and have always obtained less ozone in the valleys and low grounds than on the hills and highlands. The amounts obtained have increased according to the height at which I have placed tests. All my experiments were made around Windermere. These facts simply show that elevated regions are more conducive to the production of ozone than low lying districts, however free from population or near the sea.

Third.—*Times when variable amounts of ozone are found.* On looking carefully through my registers for the last five or six years, I find that, as a rule, the amounts of ozone increase as the height of the barometer decreases. It is well known that the largest amounts of ozone are detected when high moist winds prevail, and during magnetic and electric storms; but when it is considered that winds are a consequence of the removal of atmospheric pressure, it may be expected that we shall find atmospheric conditions answering to the reduced amount of pressure. As the amount of atmospheric pressure is less on high than on low lands, and as more ozone is found on high than on low lands, so if all other conditions remain the same, it will happen that as the atmospheric pressure is reduced on the low lands a proportionate increase of amount of highland ozone will be found.

Fourth.—*The relation of ozone to oxygen.* It is well known that if oxygen be eliminated from water by means of electric currents, ozone is found in the oxygen so eliminated. The force or activity thus given to oxygen causes it to manifest ozone in large quantities. Clearly then oxygen

and ozone are intimately related; and it is not unreasonable to suppose that oxygen is more easily rendered active at a high position in the atmosphere than a low one, that is, rendered more active under a light pressure than under a dense one.

Fifth.—*The apparent connexion of Ozone with Solar Radiation.* Whether we liberate oxygen chemically, or by electricity, the force which produces heat has to be employed. The source of terrestrial heat is the sun; and this heat, in the main, is the cause of all our atmospheric phenomena, especially of those movements of the air called winds, and the magnetic and electric storms the great companions of ozone manifestations. Two important papers have been read before this society by Mr. Josh. Baxendell, F.R.A.S., "On Solar Radiation," which have been printed in the 7th vol. of the proceedings of the Society. In the first of these papers it is shown—

"1st. That the calorific intensity of the sun's light is subject to periodical changes, the maxima and minima of which correspond respectively with those of the solar spot frequency."

"2nd. That the intensity of a ray of direct sun-light on its arrival at the earth's surface in the latitude of Oxford is greater in April and September than in June, when the sun's meridian altitude is greatest.

"3rd. That the curve representing the mean monthly values of solar radiation on cloudless days, has its times of maxima and minima corresponding with those of the curve representing the mean monthly diurnal ranges of the magnetometer."

And at page 105, Mr. Baxendell further shows "That clouds and haze are less prevalent during the day, or their power to intercept the heating rays of the sun is less active in the spring and autumn than in the winter and summer months." In the first of these papers, Mr. Baxendell gives

the mean monthly differences between the maximum temperature in the shade and that in the sun during the six years 1859 to 1864. They are as follows :—

January	5·51°	July	13·81°
February	9·16°	August.....	14·03°
March	10·08°	September	13·71°
April	13·23°	October	9·66°
May	12·90°	November	8·00°
June.....	12·93°	December.....	4·50°

Here are represented two maxima of solar radiation which occur in April and August, and two minima in May and December. Below I represent the mean monthly amounts of ozone for five years, from 1862 to 1867 :—

January	1·15°	July	1·29°
February	1·44°	August.....	1·34°
March	1·85°	September	1·01°
April	2·51°	October	0·80°
May	2·00°	November	0·88°
June.....	1·48°	December.....	1·25°

From the foregoing table two maxima will be seen identical with those of Solar Radiation, viz. one in April and another in August. The minima are not satisfactory. There are reasons for this. The first four years' observations were made near the village where the amounts, for the reasons already given, were small and fitful; and the test papers used were not so sensitive as those I have since used. The amounts I have obtained for the last year and a half are more than double the amounts I obtained at my previous station. I am much farther from the village, several feet higher, and use very sensitive test papers. I anticipate that future observations at my present station will far more fully concur with the Solar Radiation deductions above quoted. In proof of this, I give below the mean monthly amounts of ozone for 1867.

January	2·74°	July	1·48°
February	3·40°	August	1·42°
March	3·57°	September	1·96°
April	5·60°	October	1·48°
May	2·29°	November	1·00°
June	1·86°	December	2·70°

But there is a still more striking relationship between the amounts of Solar Radiation and Ozone. In Mr. Baxendell's second paper on Solar Radiation at page 104, we are presented with the reduced annual values of Solar Radiation at Eccles, together with their Ratios, as follows :—

Year.	Reduced Values of Solar Radiation.	Ratios.
1863	9·82	1·63
1864	9·84	1·51
1865	8·32	1·57
1866	7·06	1·17

After this is added, for comparison, the number of groups of solar spots observed in each year, by Schwabe, to which I have added a column giving the mean annual amount of ozone in each of these years :—

Year.	Ratios of Solar Radiation.	Mean Annual Amount.	Number of Groups of Solar Spots.
1863	1·63	1·55	124
1864	1·51	1·12	130
1865	1·57	0·95	93
1866	1·17	0·97	45

Here it is obvious that the amounts of ozone have decreased each year with the amount of solar radiation, and pretty nearly too as the groups of solar spots have decreased.

Now, if it be determined that the intensity of the solar radiation is coincident with the periodic activity of the solar photosphere—which I think Mr. Baxendell's papers largely, if not conclusively, confirm—and if we find a

periodicity of ozone intensity nearly identical with the periodicity of solar radiation, there is a warranty for concluding,—

1st. That ozone is the result of an activity produced in oxygen by the direct action of solar radiation.

2nd. That as a highly compressed, dense, or confined atmosphere impedes the effects of direct solar radiation, so less ozone is found in valleys and low-lying districts than on mountains and highlands.

3rd. That as direct solar radiation is impeded in large towns and populous districts, and the action of oxygen is in many ways neutralized, therefore little or no ozone is found in such places.

"Notes of Comparisons of Ozone Papers," by G. V. Vernon, F.R.A.S., F.M.S.

Mr. Mackereth, F.R.A.S., having kindly supplied me with papers prepared by Dr. Moffat's method, but on unsized paper, I thought it would be interesting to see what the relative amounts of ozone found would be as compared with Dr. Moffat's ordinary papers. The papers were exposed in precisely a similar manner, and quite free from any solar influence.

The observations only extending over a few months, there is of course but little to be determined from them so far, except the fact of the much greater amount of ozone shown by the papers prepared from unsized paper. With these papers amounts are frequently registered when the sized papers give no indication whatever, and in addition to this the average daily amounts are much greater. The details I have annexed in Table I.

TABLE I.

	Moffat Old.		Moffat Unsized.		Excess of Days of Unsized Paper.
	Days.	Mean Daily Amt.	Days.	Mean Daily Amt.	
1867.					
August	14	2·26	15	3·48	1
September.....	13	1·80	15	2·71	2
October	7	0·93	12	2·59	5
November	3	0.19	6	1·22	3
December	5	0·52	10	1·84	5
1868.					
January.....	19	3·06	19	4·97	0
February	19	1·80	24	5·40	5
March	18	2·30	22	6·20	4
April	11	1·56	14	3·80	3
Sums	109	...	137	...	28

TABLE II.

	Sized Papers.	Unsized Papers.	
			Ratio.
1867.			
August	2.26	3.38	1·49
September.....	1·80	2·77	1·54
October	0·93	2·26	2·43
November	0·19	0·60	3·13
December	0·52	1·26	2·42
1868.			
January.....	3·06	4·97	1·62
February	1·80	5·03	2·79
March	2·30	5·77	2·50
April	1·56	3·20	2·05
Mean.....	...	...	2·22

In Table II. I have given the mean daily amounts with each kind of paper, omitting, however, from the observations with unsized paper all those days upon which the sized paper gave no indications. In the last column of this table are given the ratios between the amounts given by each paper in the previous columns, in order to see the relative values of the indications. The mean ratio appears to be 2·22 for the period in question, showing that the observations made with the old Moffat papers require multiplying by this amount to correspond with the new

papers. These ratios seem to vary a good deal, and do not appear to depend upon the amount of ozone observed.

Extract of a letter received by the Editor from Mr. Vernon, F.R.A.S. May 7th, 1868:—

As bearing upon a point in the paper read by Mr. Mackereth, F.R.A.S., on ozone observations, I may state that between 1856 and 1862 I found from observations made at Newby Bridge, Lancashire, and the summit of a hill at the same place having a difference of about 500 feet of altitude above the sea, that the amount of ozone at the higher station was invariably greater than that found at the lower one. This I considered was owing to the upper station being much more exposed to strong atmospheric influences. This corroborates strongly what Mr. Mackereth has found in the neighbourhood of Windermere.

It is very unfortunate that we have no ozone observations round our eastern coasts, as that would tend to solve the question as to the very small amounts found during the prevalence of easterly winds, and we should then be able to see if an easterly sea breeze developed it. Some of the favourite watering places on the Yorkshire coast — Scarborough, Whitby, Filey, Flamborough Head, &c.—would just be the places for making these observations.

"Note on the Correction of an Algebraic Solution," by the Rev. Thomas P. Kirkman, M.A., F.R.S., Hon. Member of the Society. Received by the Editor June 8th, 1868.

An important step in the reasoning is omitted at p. 147. It is not true that the four functions in lines 2, 4, 8, and 9 are rational functions of the coefficients of G=0. They are all five-valued functions of $x_1x_2\ldots x_5$, and their values are are first to be formed on the following five shapes of our

group of 60 positive substitutions, of which the first only is expanded in page 145, each group to be followed by its negative derivate of 60,

12345	12345	12345
23145	21435	23451
31245	34125	34512
	43215	45123
		51234

12345	12345	12345	12345	12345	12345
23145	21543	23451	14352	21354	23451
31245	35142	34512	15324	45312	34512
	53241	45123		54321	45123
		51234			51234

12345	12345	12345	12345	12345	12345
12453	32154	23451	12453	13254	23451
12534	42513	34512	12534	14523	34512
	52431	45123		15432	45123
		51234			51234

The specific difference of these five forms of the same group of $3\cdot4\cdot5=60$ lies in the group of $3\cdot4=12$. The four functions in lines 2, 4, 8, 9 of p. 147 are to be constructed in all their five values on these five forms.

Page 143, lines 21 and 24, for *ambiguous* read *unambiguous.*

„ 141, over C, for 4123 read 4231 ;

„ 144, line 14, for $a(x_5-x_2)$ read $a(x_4-x_2)$;

„ „ line 15, for $a(x_6-x_3)$ read $a(x_5-x_3)$;

„ „ line 17, for *cube* read *sixth.*

P.S.—Sept. 7. I have to confess that my attempt in the preceding number to solve the quintic, &c., is, like all such efforts, a failure. My friend, Professor Harley, has proved to me that N_i, P_i, &c., pages 142, 143, though symmetrical in $x_1x_2x_3x_4$, are not so in $x_0x_1x_2x_3x_4$, in spite of the cyclical character of the values of H. But, I trust that what I have written is not utterly useless in connection with the values

of functions, nor do I despair of its leading to the coveted solutions.

I have been asked for my proof that $P + Q$ must be symmetrical in the z's, if $P^5 + Q^5$ is so, page 133. I say that if $P^5 + Q^5$ is symmetrical, P^5 cannot have more than two values by the permutations of the z's, viz., P^5 and Q^5. If $P + Q$ is not symmetrical, P will have more than two values by such permutations, and every substitution among the z's which turns P into P_1 or P_2 will turn PPPPP into $P_1P_1P_1P_1P_1$ or $P_2P_2P_2P_2P_2$, *i.e.* P^5 will have more than two values.

"Note on the Resistance of Fluids," by Dr. J. P. Joule, F.R.S., President. Received by the Editor, July 21st, 1868.

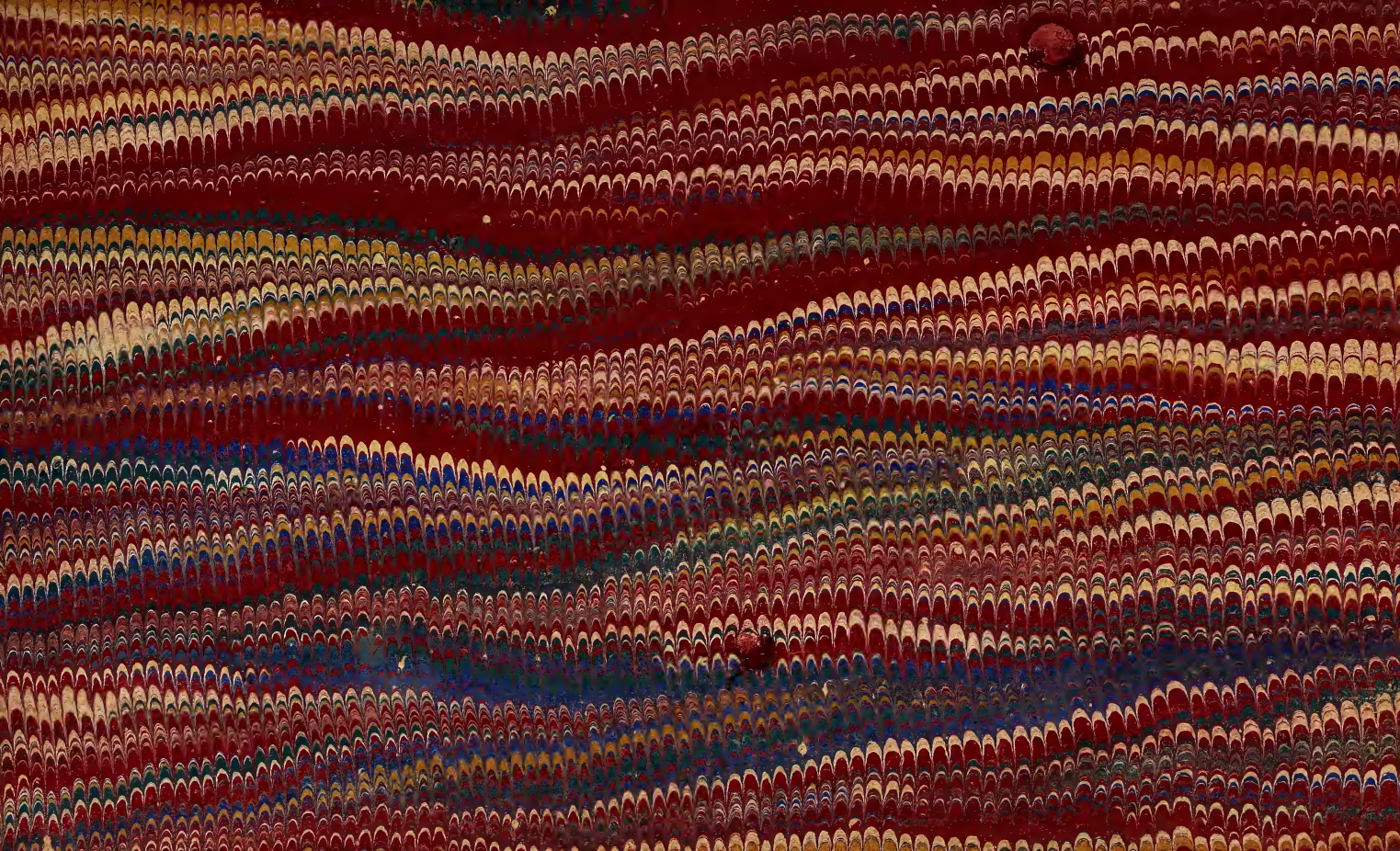
Coulomb appears to have been the first who pointed out clearly the two sources of resistance to bodies moving in fluids, the first varying in the simple ratio, the second in the duplicate ratio of the velocity. He found the simple ratio to prevail only at the very slow velocity of not more than half an inch per second. I have long had in view experiments for the purpose of further illustrating this subject, and possess an apparatus which, although constructed so long ago as 1849, I have not experimented with before the last few weeks. It consists of two vertical discs of turned and polished steel, each 18 inches in diameter, which revolve between corrugated iron plates at about one quarter of an inch distance. The axle passes through a leather collar, and the weight of the discs, which is considerable, is supported by friction wheels. Revolution is effected by the agency of weights and pulleys. A train of wheels in connection with the axis, by which a papered drum is moved under a pen worked by a pendulum, furnished the means of measuring the velocity. The following are the results at which I have already arrived. They are referred to one disc :—

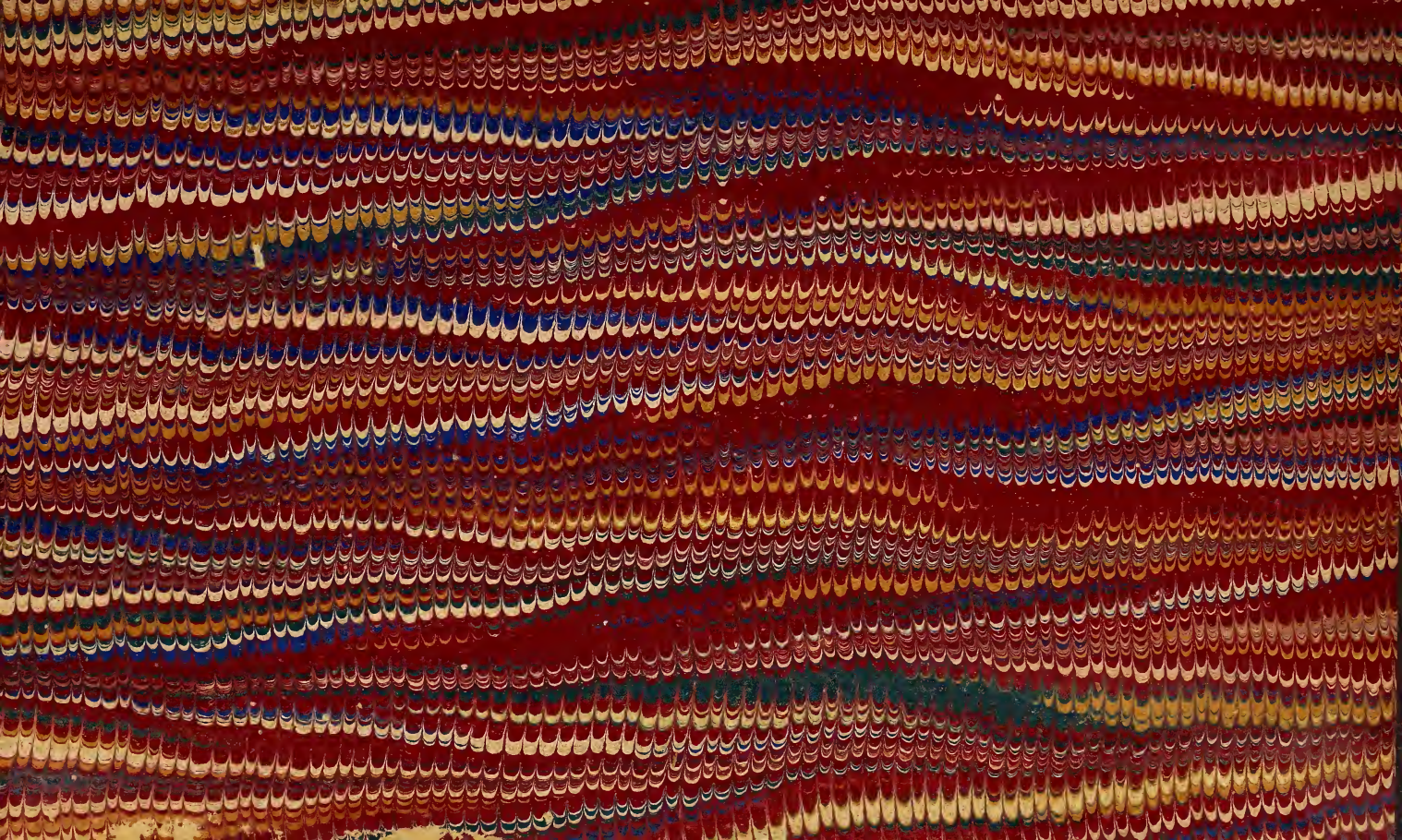
Fluid.	Velocities of Rim in feet per second between which the Experiments were made.			Index of the Power of the Velocity which is proportional to the Resistance.
Air	·7	and	5·36	·64
	5·36	„	13·2	·98
	13·2	„	23·03	1·7
Water	·47	„	5·2	1·084
	5·2	„	13·44	1·78
Oil	·19	„	1·78	1·24

I infer from the above that in the circumstances of my experiments the resistance of liquids up to five feet per second is nearly in the simple proportion of the velocity. The following are my results as to the resistance due to a given velocity :—

Fluid.	Pressure on Rim in grains.	Velocity of Rim.
Air	853	23
Water	6267	13·44
Oil	6715	1·79

When the liquids were exposed to three atmospheres' pressure, the resistance appeared to be slightly increased. I hope to clear up this and other points, and to obtain more exact results, when the apparatus shall have received the improvements which have suggested themselves in the course of this preliminary enquiry.





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